PROCEEDINGS

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NINETY-SECOND SESSION.

Monday, 3d May 1875.

DAVID MILNE HOME, Esq., Vice-President, in the Chair.

The Portrait of Sir ROBERT CHRISTISON, Bart., Honorary Vice-President of the Society, executed by George Reid, Esq., Aberdeen, was uncovered.

The following Communications were read:—

1. Laboratory Note.—Analysis of Titaniferous Iron Sand from North Berwick. By James Davidson, Esq. Communicated by Professor Crum Brown.

In an excursion of Professor Geikie's class to North Berwick (5th February), a layer of titaniferous iron sand was found lying along the shore for a short distance.

The rocks at this locality consist chiefly of dull green and red trap-tuffs, traversed by dykes and veins of basalt. Immediately in front of the spot where the iron sand was met with several intrusive masses of dark heavy basalt occur. It was evidently from the decomposition and trituration of this rock that the iron sand came, which has been assorted by the waves along the upper margin of the beach.

It was analysed with the following result:—

Magnesia	0·2
Ferrous oxide	17·6
Titanic acid	6·2
Ferric oxide	75·6
	99·6

Specific gravity 4·6.

The sand was highly magnetic.

Assuming that the magnesia is present as titanate of magnesia, and that the titanic acid, in excess of what is required to form titanate of magnesia, is united with ferrous oxide to form ferrous titanate, the residue of the ferrous oxide being united with ferric oxide, we obtain the following proximate composition:—

MgO, TiO_2	0·6
FeO, TiO_2	10·6
FeO, Fe_2O_3	42·9
Fe_2O_3	45·5
	99·6

The sand is obviously crystallised, but the grains are so rounded by rolling as to render it impossible to determine whether they are octahedral or rhombohedral.

If we calculate the quantity of oxygen contained in the several oxides present, we find,—

		Oxygen.
MgO	0·2	0·08
FeO	17·6	4·14
TiO_2	6·2	1·21
Fe_2O_3	75·6	22·68

Or arranged as above.

		Oxygen.
MgO, TiO_2	0·6	0·24
FeO, TiO_2	10·6	3·42
FeO, Fe_2O_3	42·9	12·00
Fe_2O_3	45·5	13·68

Dividing into oxides having the general formula M_2O_3 and M_3O_4 respectively (Fe_2O_3, $MgTiO_3$, $FeTiO_3$; and Fe_3O_4) we have,—

Oxygen in M_2O_3 . . .	17·34
„ „ M_3O_4 . . .	12·00

These numbers are nearly in the proportion of 3:2, and the composition of this sand might be nearly represented by the formula, $2\,M_2O_3, M_3O_4$.

The composition, however, nearly agrees with that of sands held by Rammelsberg, with good reason, to be mixtures.

2. On some Permian Fishes, hitherto erroneously referred to the Genus *Palæoniscus.* By Dr Traquair.

3. Note on the Action of Bile Salts on the Animal Economy. By J. Graham Brown, Esq. Communicated by Dr M'Kendrick.

The investigations recorded in this paper were undertaken for the purpose of elucidating the action of the bile salts on the animal economy. The chief workers in this field of inquiry have been Frerichs and Kühne, and the results obtained have been to a great extent contradictory.

The most important conclusions at which Frerichs arrives, are:—(1.) That injections of bile into the blood of the lower animals are followed by no important derangement of the vital functions; (2.) That bile pigment is excreted in considerable quantity by the urine after injections of decolorised bile; (3.) That unchanged bile acids are never found in the urine after such injections.—("Clinical Treatise on Diseases of Liver," vol. i. p. 394.)

Kühne, on the other hand, affirms with an equal degree of positiveness:—(1.) That biliary acids are not decomposed in the blood. In whatever manner they find their way into that fluid they are afterwards excreted unchanged by the kidneys; (2.) After injections into the veins of colourless solutions of bile salts, bile pigment may appear in the urine (as stated by Frerichs), but it is due to the property possessed by the bile acids of dissolving the blood corpuscles, and thus setting free a quantity of hæmatine, which being acted on by the bile acid is converted into bile pigment.—(*Vide* Beale's "Archives of Medicine," vol. i. p. 342.)

Neither of these observers notices any abnormal *post mortem* appearances in any of the animals they operated on.

In the prosecution of this investigation there were various courses open by which bile might be introduced into the system. It was not desirable to administer it by the mouth, for then its absorption into the portal circulation, and consequent passage to the liver, would tend to complicate its action. It seemed therefore best to introduce the bile directly into the general circulation by hypo-dermic injection. Intravenous injection was not employed, in order to avoid the error of some who, injecting biliary fluids into the veins, attributed the immediate effects produced on the system to the bile, forgetting that the intravenous injection of any solution, or even of distilled water, will produce such symptoms.

Having then decided to inject the bile into the subcutaneous cellular tissue, it remained to consider what preparation was best for this purpose. Frerichs used filtered bile, but this, though the simplest way, was open to many objections.

It seemed more advisable first to separate from fresh bile the salts of the biliary acids, and to inject these in solution in water. The method of preparation of these salts corresponded very closely with that described by Dr Lauder Brunton in the "Handbook for the Physiological Laboratory." In this manner a mixture of glycocholate and taurocholate of soda is obtained, consisting of nearly equal quantities of these salts. This mixture was dissolved in distilled water so as to make a solution of known strength. In these experiments the solution used contained gr. j. in ♏iij.

The rabbits experimented on were placed in a cage, the floor of which was constructed of glass rods, lying parallel to each other, and very close, and which allowed the urine to pass through into a porcelain dish placed below for its reception, while it retained in the cage the solid excreta. The urine was thus obtained in a very pure state, as it passed through the floor of the cage immediately on being voided. Its quantity was measured in cubic centimeters, as this rendered future calculation much more expeditious.

The urine was tested in the usual way for albumen, bile acids, bile pigment, and blood. The estimation of the *urea* was attempted by two methods (the one to check the other): *Firstly*, by Liebig's process with nitrate of mercury; and *secondly*, in an easier manner by the use of hypobromite of soda (as recommended by Dr Graham Steele, in the "Edinburgh Medical Journal" for August 1874). These two methods invariably gave results that corresponded very closely, and thus led to greater certainty. But, unfortunately, in the case of both of these processes, other urinary constituents come in to complicate the results of analysis. For not only does *Urea* precipitate the mercury solution, but uric acid, hippuric acid, creatin, creatinine, and even perhaps fibrin, cause precipitation. The same is true with respect to the process with the hypobromite of soda, as these bodies all unite with urea in giving off nitrogen. It is evident, then, that it is impossible by either of these methods to obtain an *absolute* knowledge of the quantity of urea, especially in the urine of herbivora, which contains so much hippuric acid. It was only possible to determine the quantity of nitrogen obtainable from the urine by means of the second process, and to use the other merely as a check. The diet of the animals experimented on was always uniform, consisting of cabbage.

And now, in describing these experiments, it will be best to speak first of those symptoms noticed during life, and second, of the *post mortem* appearances. The former will be given under the system to which each belongs, so that they may be arranged methodically.

SYMPTOMS DURING LIFE.

Alimentary System.—No salivation was ever noticed. The appetite was much affected, sometimes almost lost if the dose was large (grains xl.), but with smaller doses (grains x.) it was not much influenced. There was never any vomiting. The bowels were not affected if the dose was small, but generally there was profuse diarrhœa. In all cases the fæces were well bile-stained.

Circulatory System.—The cardiac pulsations were, in most cases, slightly but quite perceptibly decreased in rapidity, a short time after injection. The respirations were similarly affected.

Lymphatic System.—In one case the blood was carefully examined, the greatest precautions being taken to prevent contact with the air as far as possible. In this case from day to day the coloured corpuscles were seen to have their form altered, while the white increased relatively in number. At the end sometimes more than 40 white corpuscles were to be seen at once in the field of the microscope.

Integumentary System.—No jaundiced line was ever noticed in the skin or conjunctivæ.

Urinary System.—The quantity of the urine was sometimes greatly increased after injection of bile salts, and at other times diminished. This difference depended, apparently, on the amount of food taken during the day following the injection. For when the sole food of an animal consists of a vegetable containing so high a percentage of water as cabbage does, the quantity of urine varies very much according to the appetite; when, however, the quantity of food remained the same, the bile acids seemed to have an undoubted diuretic effect.

The *colour* of the urine in health was almost invariably light. It was opaque owing to a deposit of phosphates. After injection if the quantity was increased, then it became still lighter in colour, but when it was diminished, it became very dark and smoky. This colour was no doubt owing partly to concentration, but its smoky colour led to the belief that some blood pigment was present. This was rendered all the more probable by the fact that blood corpuscles were once or twice seen in the urine after injection of bile salts. However, the spectrum of the urine did not show the characteristic absorption bands of blood on the two occasions on which it was in my power to apply this test.

There once appeared a trace of albumen after injection of bile salts. Small quantities of bile acids could usually be detected after injection, but only rarely was there any trace of bile pigment; it must be confessed, however, that the test for the latter is much less satisfactory than that for the former. The quantity of *free nitrogen** almost always rose after injection of bile salts, as seen in the following tables:—

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* By free nitrogen is here meant the nitrogen which is liberated by the action of hypobromite of soda.

First Injection. (Grains x. of bile salts.) October 6, 1874.

	Total Nitrogen obtained.	
Average of 5 days (previous to injection),	510·6	cubic centimeters.
First 24 hours after injection, . .	679·95	,,
Second ,, ,, . .	436·0	,,

Second Injection. (Grains xxv.) October 9, 1874.

	Total Nitrogen obtained.	
Average of 2 days (previous to injection),	452·37	cubic cents.
First 24 hours after injection, . .	676·66	,,
Second ,, ,, . .	723·75	,,
Third ,, ,, . .	592·665	,,
Fourth ,, ,, . .	643·5	,,
Fifth ,, ,, . .	486·6	,,

Third Injection. (Grains viii.) November 2, 1874.

	Total Nitrogen obtained.	
Average of 6 days previous to injection,	456·25	cubic cents.
24 hours after injection, . . .	356·25	,, (Death.)

(Perhaps the diminution of nitrogenous excretion in this case may be explained in some extent by the rapid death of the animal.)

Fourth Injection. (Grains viii.) November 16, 1874.

	Total Nitrogen obtained.	
First day, previous to injection, . .	673·0	cubic cents.
Second ,, ,, . .	467·6	,,
Third ,, ,, . .	474·2	,,
Fourth ,, ,, . .	531·2	,,
Fifth ,, ,, . .	591·6	,,
Sixth ,, ,, . .	300·0	,,
First 24 hours following injection, .	449·3	,,
Second ,, ,, . .	440·0	,,
Third ,, ,, . .	288·0	,,

In this fourth injection there was some diarrhœa on the 5th day, and this reduced the nitrogenous excretion on the 6th day, and so the rise after the injection is not so well seen.

Fifth Injection. (Grains xxiv.) November 21, 1874.

	Total Nitrogen obtained.	
Average of 3 days previous to injection,	333·36 cubic cents.	
First 24 hours after injection, . .	666·38 „	
Second „ „ . .	763·14 „	
Third „ „ . .	945·0 „	
Fourth „ „ . .	881·9 „	(Death.)

The weights of the animals used for these injections were as follows:—

First injection, . . .	2 lbs. 2 oz.
Second „ . . .	2 lbs.
Third „ . . .	1 lb. 1¾ oz.
Fourth „ . . .	1 lb. 7 oz.
Fifth „ . . .	1 lb. 4 oz.

In the 6th and 7th injections there was such watery diarrhœa that the urine could not be collected separately, and thus the results of analysis were not to be depended upon. In the other cases the urine was not submitted to nitrogenous analysis.

It might be stated as an objection, that as the bile acids were found in the urine, and as they contained much nitrogen, they gave off this nitrogen when treated with hypobromite of soda solution, and thus increased the nitrogen of the urine. No doubt this is true, but only to a very limited extent; for when the biliary solutions used were separately analysed to ascertain how much nitrogen they gave off, it was seen to be so small, that even were the whole solution injected excreted during the next twenty-four hours, it would be utterly insufficient to account for so great a rise in the quantity of nitrogen as was seen to take place after the 5th injection.

Nervous System.—Each injection of bile salts is usually followed by drowsiness. In cases which are to end fatally this deepens, and the animal at last becomes almost comatose. In this state the animal sleeps nearly the whole day, rouses up if touched, but merely moves a little, and again falls asleep. Convulsions were never noticed, but they might have been overlooked, as the animals never happened to be under observation at the time of death.

The pupil never showed any particular change.

Let us now turn to a consideration of the *Pathological appearances* noticed after death. Out of four fatal cases these appearances were only seen in three, as the other animal died so rapidly as not to permit of their development. The lesion seemed to be confined to the liver and kidney, and was of the following nature:—

The Liver was of natural size, but congested, and throughout its substance were scattered numerous small white patches, which contrasted well with the dark red colour of the rest of the organ. The borders of these patches were seldom or never well defined, but the lesion seemed to affect particular lobules. On removing a small piece from one of those white portions of a fresh liver, and teasing it out, one could easily see with the microscope that the liver cells were altered, swollen, with an irregular outline, and full of minute, highly refractive granules. The nucleus was in some cases obscured, in others more distinct than usual. There could also be seen what were evidently the remains of hepatic cells, a nucleus surrounded by a mass of fine oil globules, with perhaps the trace of a cell-wall on one side. If, on the other hand, a portion of the more healthy tissue be examined, liver cells approaching very closely to the normal can be seen.

After hardening in alcohol, or in chromic acid ($\frac{1}{8}$ %), sections of this liver structure may be made, but the changes described are not nearly so well seen then as in fresh specimens. This change in the hepatic tissue is not merely a fatty infiltration, such as takes place, to a limited extent, normally after each meal, but a true fatty *degeneration*. Had it been mere infiltration the lesion would have been general, and not confined to particular regions of the organ; the oil would have been in larger globules, and not in the fine molecular form seen in these specimens; and the hepatic cells would not have been broken down.

The Kidneys were of natural size, but rather pale and flabby. When a transverse section is made across the tubules, the epithelium lining then is seen to be granular and swollen, sometimes to such an extent as to block up the tubule. In order to distinguish clearly that the obstruction in the tubules is composed of swollen epithelium, it is necessary to prepare the sections and render them transparent by treatment with oil of cloves. When this has been lone the epithelial cells lose their granular appearance, and their outlines can be more readily traced. In a favourable specimen you may then see clearly the slight spaces between each cell converging towards the centre of the tubule, the interstices taking somewhat the appearance of a leech bite.

In other tubules the same appearance may be seen in a more modified degree—the cells swollen, but not to such an extent as entirely to block up the tubule. In other parts of the section tubules may be seen entirely denuded of their epithelium. In longitudinal sections these appearances cannot be so well seen.

A consideration of these facts leads to the following conclusions:—

I. That a mixture of glycocholate and taurocholate of soda when injected hypodermically, in rabbits, in doses of 40 grains and under, does not cause any immediate disturbance, but is almost always fatal (unless the dose be small) in a period varying from 30 hours to 3 or 4 days.

II. That such injections often cause an increased nitrogenous excretion by the urine.

III. That such injections frequently cause diarrhœa.

IV. That such injections are followed by an excretion of a small amount of bile acid by the urine invariably, and in some cases that bile figment is also so excreted.

V. That such injections are followed by a fatty degeneration of the hepatic secreting cells, and of the renal epithelium.

VI. That such injections cause a destruction of red blood corpuscles, and consequently an apparent increase of the white.

VII. That such injections are frequently followed by drowsiness and somnolence.

It would be out of place to attempt to give here a detailed account of the bearing which those results may have on liver disease. It may, however, be right to indicate in a brief way a few of those points which seem to be of special clinical interest.

And, first, in regard to *phosphorus poisoning*, where the liver, kidneys, and muscular fibre become fatty. No true analogy can be traced between the results of these experiments and this disease, because in poisoning with phosphorus the lesion is fatty infiltration, and not degeneration (according to Niemeyer). There is then only an apparent, not a real similarity between the two conditions.

Disintegration of the hepatic tissue has been occasionally observed to follow obstruction of the bile duct. Frerichs has recorded such a case, but he does not think that this result can be owing to retention of bile in the system; for in some cases where the bile duct was undoubtedly obstructed, disintegration of the hepatic tissue did not occur. But it should be remembered that the bile, though prevented from reaching the intestine, may yet be excreted by the urine. Hence in cases where the kidneys are acting rightly no poisonous effects need be produced. But if, owing to any cause, the urinary secretion be interfered with (as seems to have been the case in the instance Frerichs refers to), then the bile will accumulate in the system, and produce its toxic effects. In cases of jaundice, Graves states* that he was always uneasy as to the issue when nervous symptoms showed themselves,—symptoms, moreover, which he remarked were often *coincident with a diminished secretion of urine.*

But perhaps the most important bearing of the results of these experiments is upon *acute yellow atrophy of the liver.* In this disease there is a rapid fatty degeneration of the hepatic tissue, and of the epithelium of the kidney. In some cases the fatty change is universal through the whole of the liver substance, but in other cases the altered appearance is only seen in *isolated portions.*

Now it seems clear from these experiments that the mere retention of bile acids in the system will, if long enough continued, produce exactly similar changes in these organs. It may be premature to argue from this that the appearances seen in acute yellow atrophy are invariably produced by biliary retention, but that they *may be* so produced is certainly true, and so far as these experiments go they point to that conclusion.

Assuming this to be correct, it will be for consideration how far the jaundice, seen in this disease, may be the *cause* and not the consequence of the hepatic lesion, and whether the efforts of the physician should not be directed more strenuously than heretofore towards the rapid removal from the system of the accumulated bile salts.

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* Trousseau's "Clinical Medicine," vol. iv. p. 305.

N.B.—While this investigation was being carried out, and after the first specimen of the peculiar fatty degeneration had been obtained, there appeared in Robin's "Journal de l'Anatomie et de la Physiologie" for December 1874 an account of similar experiments by MM. Felz and Ritter, professors in Nancy. These gentlemen remarked the same structural changes in the liver and kidneys as have just been described. They also noticed the diarrhœa which follows the injection, and a slight increase in the nitrogen eliminated by the urine. On some points, however, their results are at variance with those recorded in this paper.

While the priority in this investigation belongs without question to MM. Feltz and Ritter, yet it will be seen from the dates of the above experiments that the most important results were obtained previous to the appearance of their valuable paper.

4. Preliminary Note on the Anatomy of the Pia Mater.

By Dr J. Batty Tuke.

5. Note on the Physiological Action of Light. By James Dewar, Esq., and Dr M'Kendrick.

The following Gentlemen were elected Fellows of the Society:—

John Christie, Esq., Cowden, Dollar.
James Thomson, Esq., LL.D., University, Glasgow.
Michael Scott, Memb. Inst. C.E.
William Jack, M.A., Glasgow.
James Bryce, M.A., LL.D.

A ballot having been taken, Dr Alexander Wood was re-admitted a Fellow of the Society.

Monday, 17*th May* 1875.

PROFESSOR KELLAND, Vice-President, in the Chair.

The following Communications were read:—

1. On the Expiatory and Substitutionary Sacrifices of the Greeks. By Dr Donaldson.

The author gives the results of his examination of the subject in the following propositions:—

1. That the sacrifices of the Greeks were offered to the gods with the idea that the food and drink would gratify them, and that the other offerings would in some way or other be pleasing to them; that the common people continued to offer up sacrifices with this belief till the end of Paganism; but that as the more cultivated classes came to believe that the gods did not stand in need of food, drink, or of gifts from them, substitutions became more and more general with them.

2. That certain sacrifices were intended to appease the anger or overcome the dislike of the gods, not because any sin had been committed, but because the Greek worshipper was not sure of the disposition of the special god towards him, and believed that the wisest course was to conciliate him.

3. That no expiatory sacrifices were offered up simply to express repentance for sin in general, but they were always occasioned by some offence against some individual god or gods; that in these cases care must be taken to distinguish between the purification and the sacrifice; that in the case of deliberate murder no expiatory sacrifice was permissible, but the murderer or his descendants must suffer death; and in the case of involuntary murder, the sacrifice was of the nature of a payment of damages.

4. That there is no instance of a human sacrifice in Homeric times. That in the classical times the one or two allusions really refer to mythical times, and that there is only one instance of human sacrifice for which there is the shadow of historical evidence; that the evidence for this human sacrifice breaks down completely on close examination, and thus we have the fact that

there is no clear proof that one human sacrifice was ever offered up in Greece during the historical period. We have, on the contrary, abhorrence of such sacrifices frequently expressed. Herodotus denounces human sacrifices as an unholy deed (*πρῆγμα οὐκ ὅσιον*). Æschylus and Euripides * employ language of utmost detestation against it. The Delphic oracle calls it a foreign practice. Pausanias and Porphyrius deem it barbarous. And Sextus Empiricus, contrasting the different feelings of mankind in regard to the same acts, says of the Greeks,—"But *we* think that the temples are polluted by human blood."† The same Greek detestation of human sacrifices is embodied in the tradition that Heracles gained renown by doing away with human sacrifice in various parts of the world.‡

5. That there is no satisfactory proof that the Greeks at any time or in any place were in the habit of offering up human sacrifices. Certain rites may find an explanation in the supposition that human sacrifices were at an early period offered up; but there is no historical testimony to show that the practice ever existed. And even in the cases where the practice may by some be regarded as the best explanation of the rite, we have not a genuine Greek race. The ceremonial on Mount Lycæus was Pelasgic. And the Agrionia and the sacrifices of the Athamantidæ are connected with the Minyan Orchomenos, the seat of Pelasgic worship. So that we should have in these three cases the traditions of the worship of the race which preceded the Hellenes, if we were to base any conclusion on the unsatisfactory information which we have in regard to them. And there are really no other decided cases of what can be regarded as survivals.

6. That the writers of the third period, influenced by the belief that the ordinary gods of the Greeks were demons of savage propensities, lent a ready ear to any tale of horror connected with their worship, and that it is in these writers that we hear of the human sacrifices of the Greeks; but if we place the evidence for these

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* Welcker thinks that human sacrifices were attacked by Sophocles in his Athamas, by Achæus in his Azanes, and possibly by Xenocles in his Lycaon. —*Die Griechischen Tragödien*, vol. iii. p. 965.

† Hyp. iii. 24, p. 209.

‡ Welcker.—*Griechische Götterlehre*, ii. p. 769.

sacrifices fairly in the balance, we shall find it not so strong as that which could be adduced to prove that the early Christians killed infants, drank their blood, and indulged in indiscriminate sexual intercourse. And yet no one now believes these accusations against the Christians.

In fact, the Greeks were strangers to the idea of sin until the introduction of Stoicism, as Sir Alexander Grant has well shown in his Aristotle, and it is likely that the idea was not present to the minds of the earlier Stoics. There is therefore, as it seems to me, no analogy between the sacrifices of the Greeks and the sacrifice of Golgotha. The sacrifice of Christ is, as Dr Crawford has admirably brought out in his "Mysteries of Christianity," p. 230, "exceptional and unique." But in the deeper meaning of sacrifice, the essence of which is self-renunciation, there is a striking parallelism between most of the Greek mythical sacrifices, including also the more or less historical voluntary deaths of Codrus and Leonidas, and the sacrifice of Christ. The oracle decrees that what is noblest, and most beautiful, and most fair must perish. The noblest and the fairest offer themselves up for their country, and present to their country the most beautiful sacrifice that can be offered—a pure human soul. And in like manner the sacrifice of Christ, not indeed devoted, like the Greek sacrifices, to a single land, but offered up for the whole world, is an act of obedience to the will of God, and an infinitely grand exemplification of that self-renunciation which constitutes the essence of all true religion.

2. The Placenta in Ruminants.—a Deciduate Placenta.

By Professor Turner.

All zoologists, who have accepted the placental system of classification of the Mammalia, agree in placing the Ruminantia amongst the Indeciduata.

As is well known, the fœtal portion of the placenta in Ruminants consists of a number of distinct cotyledons. Each cotyledon is composed of numerous branched villi, which fit into pits or depressions situated in mound-like elevations of the wall of the uterus, called the maternal cotyledons. It is generally believed, that in the process of parturition in these animals, the fœtal villi are drawn out of the maternal pits without removing any of the maternal substance along with them, just as the fingers are drawn out of a glove without any portion of the substance of the glove accompanying them, so that the placenta is non-deciduate.

Having been engaged in the study of the structure of the placenta in the undelivered cow and sheep, it seemed to me, from the mode in which the fœtal villi divided into branches, and from the consequent subdivision of the maternal pits into smaller branching compartments, that the interlocking of the fœtal and maternal tissues with each other was so great as to render it difficult, if not impossible, for the fœtal villi to be forcibly expelled from the maternal cotyledons, without carrying away with them some of the uterine tissue. With the object of testing the accuracy of this supposition, I procured, in the spring of the present year, the fœtal membranes, separated in normal parturition, both of the cow and sheep, and submitted the fœtal cotyledons to microscopic examination.

Before describing the microscopic appearances, it may be advisable to say a few words on the general arrangement of the cotyledons, both fœtal and maternal, in these animals.

I shall first describe the arrangements in the sheep from a specimen where the cotyledons were well developed.

In this animal the maternal cotyledons projected as cup-shaped mounds from the uterine wall. They were covered on the outer convex surface by the uterine mucosa, which was prolonged as far as the free inverted edge of the cup. The inner surface of the cotyledon was composed of a soft, spongy material, containing numerous pits, which extended almost vertically, and divided as they passed deeper into its substance, into smaller compartments, which radiated towards the outer wall of the cotyledon, without diverging much from each other. The pits were lined by well-marked cells, most of which were irregular in shape, polygonal, ovoid, or even somewhat caudate, and of considerable size, though some few were like modified columnar cells. They consisted of granular protoplasm, in which one, two, or sometimes three, well-defined ovoid or elliptical nuclei were imbedded, but without a cell-wall. Not unfrequently the outline of the individual cells was very indistinct, and they seemed as if composed of a layer of protoplasm studded with nuclei.

The cells rested on a highly vascular sub-epithelial connective tissue, which formed the proper wall of the pits. The mucous membrane investing the cotyledon was continuous at the mouth of the cup with the walls of the pits in the spongy tissue, so that the cells lining the pits were in the same morphological plane as the epithelium covering the mucosa. The cotyledons were highly vascular. Some of the arteries in the sub-cotyledonary connective tissue were corkscrew-like; and in the deeper part of the cotyledon itself I have seen tortuous vessels. The greater number of the vessels within the cotyledon passed, however, vertically towards the surface lying in the connective tissue walls of the pits; branching repeatedly, as a rule in a dichotomous manner, prior to forming a compact maternal capillary plexus,—not dilating into maternal blood sinuses.

The mucous membrane of the uterus between the cotyledons contained numerous tortuous, branched tubular glands. Some of these extended almost vertically to the surface, and could be seen in almost their entire length in vertical sections—others ran more obliquely, and owing to their tortuosity, were repeatedly divided in vertical sections. The mouths of the glands could readily be seen with a pocket lens opening on the surface, the orifice being partially surrounded by a minute elevation of the mucosa. In the mucosa around the base of the cotyledons, a ring-like series of gland openings were seen. In the mucosa covering the cotyledons glands were also present, but their orifices were much stretched, as if by the pressure due to the great growth of the subjacent spongy tissue of the cotyledon. The sub-epithelial connective tissue in which the glands lay, was not by any means so vascular as that which formed the walls of the pits within the cotyledons. In some sections through the cotyledons and adjacent mucosa no glands were to be seen in the connective tissue intervening between the cotyledon and muscular wall, but they were collected in considerable numbers around the cotyledon, as if pushed outwards by its rapid growth. In other sections, however, tubular glands were seen in the sub-cotyledonary connective tissue; but they seemed to be the deep ends of the branching glands, the stems of which may have inclined obliquely, so as to open on the surface of the mucous membrane covering the cotyledon. None of these subjacent glands, or those situated on the surface of the cotyledon, were seen to open into, or in any way to communicate with, the pits within the cotyledon itself.

The fœtal cotyledons consisted of numerous villi, which collectively formed a ball-like mass, occupying the concavity of the maternal cotyledon. Each villus consisted of a main stem, which gave off a tuft or cluster of spatulate branches. The villi entered the maternal pits and branched along with them, so that every compartment was occupied by a branch of the villus; but there was necessarily no great divergence of these branches from the main stem. At their deeper end these spatulate branches gave off slender terminal offshoots. The villi were formed of gelatinous connective tissue, in which very distinct fusiform and stellate corpuscles were arranged in an anastomosing network. At the periphery of the villus was a layer of flattened cells, with small but distinct nuclei arranged so as to form an epithelial-like investment. The umbilical vessels ramified within the villus and formed networks of capillaries. The villi were in close contact with the epithelial cells lining the maternal pits. Owing to the inversion of the free edge of the maternal cotyledon and the radiated arrangement of the pits, with their contained villi, it was impossible to disengage the maternal and fœtal cotyledons from each other without drawing away with the fœtal villi portions of the maternal cotyledon. I invariably found that, in drawing the fœtal villi out of their compartments, flakes of epithelial cells accompanied them, which showed how readily this element of the maternal tissue is shed. During parturition, however, when the parts are relaxed, the disengagement of the two structures is necessarily more easily accomplished.

In the cow the maternal cotyledons differed in form from those in the sheep. They were fungiform or umbrella-shaped, and were connected to the uterine wall by a broad neck, around which the uterine mucosa was prolonged as far as the border of the umbrella. The whole convex surface of the cotyledon was riddled with pits, which passed vertically into its spongy substanco, and divided into smaller compartments in the deeper part of the cotyledon. Projecting from the wall of each pit were delicate bands, visible to the naked eye, arranged as a rule in a vertical direction, and in the intervals between these bands the wall was perforated by numerous orifices, easily seen with a pocket lens, which were the mouths of depressions or crypts in the wall of the pit, some lying almost at right angles, others obliquely to the wall of the pit itself. The pits, with their numerous crypts, were lined by epithelial cells, similar in character to those of the sheep, and these cells rested on a highly vascular connective tissue, in which the maternal capillaries formed a compact network. But I should state that in the cow a larger proportion of these cells had preserved the columnar form of the epithelium of the non-gravid uterine mucosa.

The surface of the uterine mucosa between the cotyledons presented the mouths of the tubular, branched, utricular glands, which extended more obliquely to the surface than in the sheep, so that in vertical sections through the membrane they were frequently cut through and divided; segments of each gland were as a rule seen, though sometimes the stem of a gland mounted to the surface to open by an obliquely-directed orifice. Glands were also present in the connective tissue forming the neck of the cotyledon, but none were seen to communicate with the pits.

The fœtal cotyledons were situated on the umbrella-shaped maternal cotyledons, and their numerous villi occupied the pits. The stems of the villi were comparatively large, and studded with multitudes of minute tufts, which, arising obliquely or almost at right angles to the main stem, entered and occupied the crypts. The minute villi forming these tufts were so slender and filiform that each terminal offshoot contained only a single capillary loop. The villi were in contact with the epithelium cells, and in drawing them out of the pits, more especially in drawing the tufts out of the crypts, multitudes of the lining epithelial cells came away with them. From the differences in shape of the maternal cotyledon in the cow and in the sheep, there is not the same difficulty in unlocking the fœtal from the maternal placenta in the former animal as in the latter.

For the purpose of studying the shed placenta of the sheep, I procured the after-birth from the ewe as soon as it was passed, and immersed it in strong spirit. Some fœtal tufts were then examined without any other preparation; but others were immersed in glycerine jelly, so as to bind the several constituents of the tuft together. Thin slices were then removed from the hardened tufts, whilst from others small portions were taken and teezed out with needles. In the examination, a magnifying power of 320 diameters was employed. Quantities of cells, having the form and appearance of the epithelial cells already described, were seen to be intermingled with the fœtal villi. In some cases small patches of cells were seen lying free in the spaces between the villi, but more frequently the cells were isolated. In a few instances I saw groups of such cells in immediate contact with the terminal villi, as if they, in being drawn out of the compartments in the maternal cotyledon, had pulled an envelope of epithelial cells along with them.

When the cotyledons of the shed placenta of the cow were examined microscopically, quantities of granular debris were to be seen floating in the fluid in which the specimens were placed. Along with these granules were small flakes of protoplasm; rounded or ovoid bodies, with distinct outlines looking like free nuclei; and large cells composed of granular protoplasm, containing one, two, or three nuclei, having the anatomical characters of maternal epithelial cells.

The amount of debris and of decidua cells varies considerably in the different slides which I examined; in some being so abundant as to render the fluid in which the specimen was examined quite turbid, whilst in others only slight traces were to be recognised.

From these observations I am of opinion that, both in the sheep and cow, the cotyledons of the fœtal placenta carry away with them, during the act of parturition, a portion of the maternal structure, so that in these animals, and presumably in other ruminants, the placenta is deciduate. So far as my observations have gone, I have only detected the epithelial element of the uterine mucosa, or the cells of the decidua serotina, intermingled with the fœtal villi; but from the bloody state of the external parts of the ewe, for some hours after the birth of the lamb, I think it not improbable that the disruption of some of the maternal cotyledons has been deeper than a mere epithelial shedding,—that the maternal vessels have, in some places at least, been torn across, so as to give rise to the hæmorrhage.

From the observations which I have made on the structure of the placenta in many of the Mammalia, both in the deciduata as well as in the so-called non-deciduata, I am of opinion that the shedding or non-shedding of maternal tissue, along with the fœtal, during the act of parturition, is determined by the degree of interlocking of the fœtal and maternal portions of the organ during the formation of the placenta, and not from the presence in the deciduata of a structure or structures which do not exist in the non-deciduata. In both forms the same anatomical elements exist, though, as in the case of the human placenta, the maternal constituents may become so modified in arrangement as greatly to obscure their original characters. The fœtal part of the placenta consists of a chorion more or less perfectly covered with vascular villi: the maternal part consists of a modified uterine mucosa, the surface of which is composed of the modified epithelial cells of the mucous membrane, beneath which is a highly vascular connective tissue, the modified sub-epithelial connective tissue of the mucosa.

In those animals in which the chorion remains almost entirely covered by villi, as in the pig, mare, and cetacean, the villi are short, with simple branches, and the depressions, pits, or crypts in the uterine mucosa for their reception are consequently shallow. During the act of parturition the villi are so readily liberated from the uterine crypts that no maternal tissue is necessarily shed along with them, though even here it is not difficult to see that, should the epithelial serotina from any cause become detached from the sub-epithelial connective tissue, flakes of it might pass off along with the villous chorion. In the zono- and disco-placentary mammals, where the villi are much longer, and, as a rule, much more extensively branched, the constituents of the mucosa, both epithelial and sub-epithelial, are so intermingled with the fœtal villi in the region of the placenta as, as is generally admitted, to be shed along with them. The ruminants, therefore, with their scattered cotyledons, are seen to occupy, as regards deciduation, an intermediate position between the animals with a diffused placenta on the one hand, and the zono- and disco-placentary mammals on the other; for whilst the former are apparently non-deciduate during the act of parturition, and the latter part with both the epithelial and the vascular sub-epithelial constituents of the uterine mucosa in the placental area, the ruminants shed, as a rule at least, only the epithelial lining of the uterine pits into which the fœtal villi are inserted.

I have hitherto spoken of the shedding of maternal tissue along with the fœtal during the act of parturition. But, to prevent misconception, it may be well to state that, as indeed has been pointed out by Owen,* by Ercolani,† and by myself,‡ in a memoir previously submitted to this Society, if not during parturition, at least afterwards, all placental mammals are deciduate; for in the pig, mare, and cetacean, "during the period of involution which follows parturition, it is obvious that great changes, either from actual shedding of portions of its substance, or from degeneration and interstitial absorption, must take place in the constituents of the crypt-layer before it can be restored to its proper non-gravid condition."

In the ruminants also, although the epithelial cells may be the only constituent of the uterine mucosa which is shed during the act of parturition, yet, after that act is accomplished, the thick, vascular, spongy tissue of the maternal cotyledon must disappear before the uterus can assume its normal unimpregnated aspect.

It will be observed that in this communication I have given to the term deciduate a more extended signification than has usually been attached to it by anatomists. It has been customary to consider a placenta as deciduate, only when both the epithelium and the sub-epithelial vascular maternal tissue are parted along with the fœtal villi.§ But it appears to me that even when the epithelial lining of the crypts only is shed, the placenta should be regarded as deciduate, inasmuch as there is a shedding of maternal structure, though, of course, in an inferior degree to one in which the sub-epithelial vascular tissue is also separated.

3. An Essay towards the General Solution of Numerical Equations of all Degrees. By W. H. Fox Talbot, Esq., Hon. F.R.S.E.

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* The Anatomy of Vertebrates, vol. iii. p. 727. 1868.

† Sur les Glandes utriculaires de l'uterus, &c. Algiers, 1869.

‡ Trans. Roy. Soc. Edinburgh, 1871.

§ Huxley—Lectures on Comparative Anatomy, p. 10. 1864.

4. Note on the Electrical Conductivity of Saline Solutions. By J. G. MacGregor, M.A., B.Sc. Communicated by Professor Tait.

In the *Sitzungsberichte* of the Munich Academy,[*] Professor Beetz has recently published a review of a paper by Mr J. A. Ewing and myself on "The Electrical Conductivity of certain Saline Solutions," which was read before the Royal Society of Edinburgh during the session of 1872–73, and is published in their Transactions.[†] I take the earliest opportunity of discussing the strength of the arguments on which his criticisms are based. Unfortunately, on account of Mr Ewing's being at present in South America, I am unable to communicate with him. I alone am therefore responsible for the contents of the present paper.

Professor Beetz begins by characterising the short reference which we made to his valuable work on the conductivity of solutions of zincic sulphate [‡] as a "complete series of mistakes." "My only precaution," he writes, "against polarisation consisted, then, in the use of amalgamated zinc electrodes! Whoever has read and understood my paper, must know that the plan of my work went beyond the experiments with zincic sulphate solutions, that it rather consisted in finding, by a damping method, the conductivity of electrolytes generally, relatively to that of a single one." We are accused of having underrated his precautions against polarisation and of being ignorant of the scope of his work. There were two things which Professor Beetz set himself to accomplish—(1.) to investigate absolutely the conductivity of zincic sulphate solutions; and (2.) to determine relatively to it that of other electrolytes. The first part he has carried out, and his researches in this field are generally regarded as authoritative. The second part, however, he has not carried out. He made use of two original and very ingenious methods for the purpose, but both failed. Of one he says himself, "*Es gelang aber nicht brauchbare Ausschläge zu bekommen;*" [§] and of the other, "*Auch diese Methode hat mir noch keine Resultate geliefert.*" [||] Now, however one may admire the ingenuity of the methods which he employed, it is impossible to speak of them as having been productive of positive results, and unwarrantable to describe them as methods which have increased our knowledge of this department of Physics. Hence we did not mention his whole plan, nor refer to all the methods by which he endeavoured to carry it out, but confined our attention to that part of it which he had successfully accomplished. If we are justified in having thus restricted our remarks to really fruitful work, we must also be justified in having given as his only precautions against polarisation, those which he adopted in his absolute measurements of the sulphate of zinc. Two sentences will show what these were:—"If we only knew a combination of liquid and electrodes, in which the electric current produces neither polarisation nor new resistances, it could be treated, so far as the measurement of resistance is concerned, exactly as a solid body." * Such a combination is easily obtainable, for "through du Bois-Reymond's investigations it is known that amalgamated zinc electrodes are not polarised in concentrated solutions of zinc vitriol." † The only necessary precaution is thus found; and, as might be expected, one looks in vain for an account of any others. Of course he made his solutions as pure as possible, excluded air, arranged his tube so that it would always be full of liquid, and kept his electrodes in the same positions; but these were not directed immediately against polarisation. His only precaution, in fact, was the only necessary one, viz., the use of non-polarisable electrodes.

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[*] Sitzungsberichte der Münchener Akademie, 6. Februar, 1875, pp. 59–70.

[†] Trans. Roy. Soc. Edin., vol. xxvii. part 1, 1872–73, pp. 51–70.

[‡] Poggendorff's Annalen, cxvii. 1862, pp. 1–27.

[§] Ibid, cxvii. p. 26.

[||] Ibid, p. 27.

In describing Professor Beetz' work, however, we fell into an error by assuming that pure zinc electrodes are not polarisable in solutions of zinc vitriol, and, therefore, regarding it as remarkable that he should have taken the unnecessary trouble of amalgamation; and the same error is seen, as he points out, in our reference to Paalzow's work, in which we say that he used pure zinc electrodes, while in reality he amalgamated them. This is, I think, the only "*Wissensfehler*" of which we can be convicted. As an error it is, of course, to be regretted. But its comparative triviality is shewn by the fact that it not only has no influence on our own work, but does not even affect our criticism of either Professor Beetz or of Paalzow. In the case of the former, we attributed to the electrodes which he used the same property as he did himself, and in the case of the latter we attributed to those which we described him as having used, the properties which belong to the ones which he really did use. Nevertheless, Professor Beetz thinks that we did not understand Paalzow's method.* "The authors," he says, "call this method 'very ingenious.' 'Curiously enough!' for they did not understand it at all. If they only had, how ingenious it would then have appeared to them." He comes to this conclusion because he thinks that, had we had a full knowledge of Paalzow's apparatus, we could not have criticised it as we did. All that is necessary, therefore, for me to prove, is that the items of description which we, for shortness' sake, omitted, do not necessitate a change in our criticism. Paalzow's apparatus, according to our description, consisted of two glasses filled with sulphate of zinc solution and joined by a bent tube containing the solution under investigation, the electrodes dipping into the zinc sulphate. Professor Beetz thinks that we had neither grasped the idea that the tube did not open into the glasses containing the vitriol, but into porous clay vessels which contained the same liquid as the tube; nor understood the meaning of his having made two or more measurements of the same solution in tubes of different lengths. Do, then, these facts destroy our criticism? We said that diffusion of the liquids must be a source of error, and that polarisation at the surface of junction might be. These dangers are not excluded even when the clay vessels are used. Diffusion may not go on so rapidly, but it still goes on; the mixture which must take place constantly changes the conductivity, and (as our experiments † shew) possibly to a great extent. Moreover, there are still surfaces of contact, notwithstanding the intervention of the clay vessels, the only difference being that instead of one large one there are numerous small ones; and there is still, therefore, the danger of polarisation ‡ without the possibility of eliminating its effects by calculation. The clay vessel not only does not obviate the two difficulties already cited; it may also introduce a new one, in the form of what du Bois-Reymond calls internal (*innere*) polarisation.* Nor is the second addition to our description more destructive of the accuracy of our criticism than the first. Professor Beetz thinks that, by measuring the same liquid in tubes of different lengths, one may take the difference of their resistances as the resistance of a column of liquid whose length is the difference of their lengths. But this can only be the case on the supposition that the state of matters at the junction of the liquids is the same for both determinations. Now diffusion does not cease at the end of each measurement and wait until the next begins. Nature is not so convenient. Every moment adds to the mixture of the solutions and changes their resistance. If, moreover, there be polarisation at the surface of contact, or (clay vessels being used) if there be also internal polarisation, it must begin at zero and increase from the first moment of contact up to the time of observation (supposing that to occur before the maximum is reached). In order that this condition may be the same for both measurements, the observations must be made after the same lapse of time from the first moment of contact, an occurrence which is manifestly improbable, and, if it should happen, impossible to know. It is true that a judicious choice of electrolytes may remove one or more, though never all, of these sources of error; but such a possibility cannot be taken into consideration in discussing Paalzow's as a general method; while, at its best, as a special method, it has always the defect arising from the mixture of the liquids. How great or how small the error arising from mixture and polarisation may be, it is difficult to say. That can only be decided by future experiments. But it is clear that the error remains, and that the method, as described most minutely, is subject to the same criticism as in its simpler form.

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* Pogg. Ann. cxvii. p. 3.

† Ibid. p. 6.

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* Monatsberichte der Berliner Akademie, 30. Juli, 1868, p 486. Pogg. Ann. cxxxvi. 1869, p. 489.

† Trans. Roy. Soc. Edin., vol. xxvii. part 1, 1872–73, p. 67.

‡ Monatsberichte der Berliner Akademie, 17 Juli, 1856, p. 1.

Passing from Paalzow, Professor Beetz proceeds to prove that we did not understand Kohlrausch and Nippoldt's † work any better than that just discussed. We ourselves are, however, to a certain extent responsible for this judgment. The cause of his misunderstanding is the careless structure of our description. By the use of a specific term in the second sentence instead of a generic, the thermo-electric currents are made to appear to have been used as a means of reducing the electromotive force of the magneto-electric currents, while in reality the former were substituted for the latter in order to obtain currents of low as well as of high electromotive force, that is, to reduce the electromotive force of the currents employed. The sentence is a parenthesis, in which the words "these currents" take, from the structure of the preceding part of the description, a narrower meaning than they were intended to have. One might almost have expected Professor Beetz to discover the defect, rather than adopt the supposition of belief in the reduction of the electromotive force of magneto-electric currents by means of a thermo-electric pair.

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* Monatsberichte der Berliner Akademie, Aug. 4, 1856, p. 15, and Jan. 31, 1859, p. 1.

† Göttinger Nachrichten, Nov. 18, 1868, p. 415. Jahresbericht des phys. Vereins zu Frankf. 1867–68, p. 71. Pogg. Ann. cxxxviii. 1869, pp. 280 and 370.

The casual remark that the resistance of either a wire or a constant cell can be easily measured, Professor Beetz translates incorrectly, and, in consequence, criticises unfairly. We did not affirm that the resistance of a galvanic cell is quite as easy (*ebenso leicht*) to measure as that of a solid body, nor did we mean to say that there is yet a perfect means of measurement, but that by Wheatstone's method (in our reference to which Galvanometer is printed Electrometer) approximately accurate results may be easily obtained. Why then have von Waltenhofen and others tried to improve upon Wheatstone? Simply because they think they can reduce the already greatly diminished sources of error, or because they wish to have a method applicable to both constant and inconstant cells.

I come next to consider Professor Beetz' criticism of the method of resistance measurement which we used; and it is interesting to notice how even he, who, having written what he has written, might be expected to take all possible precautions against mistake, nevertheless can slip into what he would call, if we had been guilty of them, *die allergröbsten Wissens- und Verständnissirrthümer*. He thinks that our method is the same as his own, except that we substituted platinum for zinc electrodes, and relied upon the quickness with which we could make and break contact for the measuring of resistance during the earliest stages of polarisation! Accordingly, his criticism is, that contact cannot be so quickly made and broken, *i.e.*, the time of passage of current cannot be made so short, as to warrant our neglecting the effect of polarisation; and he cites as proofs of the fact the experience of Kohlrausch and Nippoldt,* and the experiments of Edlund.† "If," he writes, "the authors were acquainted with Edlund's experiments, they would know that platinum electrodes, between which the current from three Daniell's cells has passed in dilute sulphuric acid during only $\frac{1}{50}$th of a second, have already acquired a polarisation, whose electromotive force is equal to that of 0·57 of a Daniell's cell." Very interesting, but unfortunately not to the point! Any one who understood the method of our paper would know that Edlund's experiments were quite irrelevant, simply because our observation was equally accurate whether the current flowed during $\frac{1}{100}$, 1 or 100 seconds. In what respect then did the method differ from Professor Beetz' idea of it? In one respect, viz., that while he supposed us to have used a heavy mirror galvanometer, we used Sir William Thomson's "Dead Beat" galvanometer‡—and we distinctly stated that such an one was necessary for the use of our method.§ This galvanometer has four peculiarities:—(1.) The mirror is exceedingly light; (2.) On the back of it there are four very small magnets;‖ (3.) The mirror cell is just large enough to admit of deflection; (4.) The front and back of the cell act as stops. In virtue of the former two peculiarities the mirror moves almost instantaneously in obedience to even a very weak current; and in virtue of the latter two, there is almost no oscillation. The effect of these properties is seen by comparing an observation made by means of the ordinary galvanometer with one made by means of the "Dead Beat." Suppose the ordinary galvanometer to be used in a Wheatstone's Bridge, one of the arms of which is a tube containing an electrolyte with platinum electrodes, while the other three are known resistances, and are arranged in such proportion that, if the tube were replaced by an equal metallic resistance, a very small deflection of the mirror, in a positive direction, would be obtained on closing the circuit. Then, during the time of contact, a large deflection is produced in a negative direction. The moment of inertia of the mirror is so great, that before the main current has moved it, at least perceptibly, in the positive direction, the polarisation current carries it off in the negative. If contact lasts $\frac{1}{50}$th of a second, the deflection is due to the sum of all the forces acting upon the mirror during that space of time. Against such a method Professor Beetz' criticism is valid; because it is almost impossible to make and break contact in less than $\frac{1}{50}$th of a second, and we certainly did not think that we had done so. But suppose that we use the "Dead Beat" galvanometer, the bridge being in the same condition. During contact the mirror makes two deflections,—the first, very small and in a positive direction, the second, much larger and in a negative direction,—the size of the second deflection depending, within limits, upon the length of time of contact, while both the occurrence and size of the first are entirely independent of it. The inertia of the mirror is so small, that the main current—lessened of course by the first traces of polarisation—produces its effect before polarisation has had time to gather its forces; and it is this first deflection, caused by the main current, which is observed, and which is reduced to just nothing by changing the relation of the arms, in order to determine the resistance of the tube. It is thus evident that the length of time of contact has no effect upon our result. So far as a single observation is concerned, it is quite the same whether it lasts a long or a short space of time. Edlund's experiments and Kohlrausch and Nippoldt's experience are thus alike worthless to a critic of our method, and Professor Beetz cites them simply because he was criticising a conception of his own. He was, perhaps, led astray by the importance we attached to making the time of contact as short as possible. But this precaution had reference to the next following observation. The shorter the contact, the less time required for depolarisation and the less change in the constitution of the liquid. The same remarks, which I have made to shew that we did not lean upon such a broken reed as the shortness of contact, make it evident also that we were right in our choice of platinum electrodes. Professor Beetz' own experience,* and that of other workers in this department, shew that platinum electrodes are the best to form part of an apparatus for the investigation of electrolytes generally.

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* Pogg. Ann. cxxxviii. p. 282, 1869.

† Ibid. lxxxv. p. 209, 1852.

‡ See Fleming Jenkins' "Electricity and Magnetism," p. 198.

§ P. 58 of our paper.

‖ The mirror and magnets of our galvanometer weighed together only about ·03 grm.

The method which we used certainly cannot be regarded as completely eliminating the effects of electrolytic action. Like the other general methods which have been tried, it is an approximation method. It aims, just as Kohlrausch's does, at measuring resistance when the effects of polarisation are so slight that they may be neglected. If the weight of the mirror were indefinitely small, its first deflection might be regarded as due to the main current alone. But it is impossible to obtain such a mirror; and the first deflection must be regarded as caused by the sum of the forces of the first few moments, which sum includes the electromotive force of polarisation. It is generally admitted, however, that polarisation, beginning at zero, must during the first few moments be exceedingly slight. The mirror, therefore, being sufficiently light, the observation will take place when its electromotive force is so small as to warrant its being neglected. The lighter the mirror, the more rapid the deflection, the smaller the influence of polarisation and the error. Whether or not, with the lightest mirror which can at present be made, the error is really small enough to be neglected, can only be determined by a comparison of results with those of some standard method which eliminates completely the effects of electrolytic action. Our method guards for a single observation not only against polarisation, but in the same way against the effect of the production of substances which, while giving rise to no new electro motive force, differ in conductivity from the original liquid. The observation is made before such products can be formed in any appreciable amount. The necessity of repeated observations in the same liquid, however, renders the elimination of this error only slightly less defective than in the methods of Kohlrausch and Nippoldt, Kohlrausch and Grotrian, Paalzow, and perhaps even Professor Beetz.

In passing to consider the numerical results which we published, Professor Beetz refers to two remarks which we made about his results, in one of which he finds the only *Verständnissfehler*, of whose occurrence in our paper I am aware. We said that in his researches on the conductivity of solutions of different density, he was not careful to keep to exactly the same temperature throughout a whole series of observations, so that his results did not admit of accurate graphic representation. This is a mistake, proceeding probably from a misunderstanding of one of his tables, and I am happy to have this opportunity of making the correction. In speaking, however, of our having made observations at constant temperatures, Professor Beetz' statements are somewhat too sweeping. He proves it himself; for the confidence which he places in Paalzow's* numbers shew that this is not the cause of his want of confidence in ours. Our experience is contrary to his opinion. We found the method of constant temperatures somewhat slow, but met with no great difficulty in making our measurements always within a small fraction of 10° C. Had it not been so, it would have been easy to adopt the method of interpolation.

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* Pogg. Ann., cxvii. p. 26.

The second remark was merely a statement of fact, and not intended, as Professor Beetz thinks, as a reproach. Which of the two kinds of formula is the better, is, of course, simply a matter of opinion. We wished merely to state that Professor Beetz, having adopted the usual one, had not availed himself of what we regarded as one great advantage of the other. He thinks, however, that we not only made the worse choice, but did not produce good specimens of the kind which we had chosen. "It would be better," he says, "if there was more agreement between their observed and calculated numbers." As a comment upon this remark, I give the tables of conductivity (observed and calculated) of the solutions which both he and we have examined. His results we take from the table given in his paper,† the latter part of which has been omitted, because our formula (a manifest disadvantage) does not apply to solutions beyond that of maximum conductivity. Our numbers are those given in our agreement-table,‡ reduced to the same form as his, the first four being omitted because they correspond to solutions weaker than any that Professor Beetz examined. All the given numbers must be multiplied by 10^{-9}.

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* Monatsberichte der Berliner Akademie, 30. Juli, 1868, p. 488.

† Pogg. Ann. cxvii. 1862, p. 20.

‡ Trans. Roy. Soc. Edin. xxvii. part i. 1872–73, p. 64.

Beetz' Table.			Ewing and MacGregor's Table.		
Observed.	Calculated.	Difference.	Observed.	Calculated.	Difference.
2387	2315	+72	1876	1883	− 7
2864	2864	0	2264	2264	0
3417	3408	+ 9	2828	2828	0
3921	3992	−71	2969	2997	−28
4450	4487	−37	3145	3166	−21
4502	4502	0	3264	3298	−34
4528	4545	−17	3344	3344	0
4594	4615	−21	3367	3379	−12
4638	4621	+17			
4641	4630	+11			
4626	4638	−12			
4628	4641	−13			
4632	4649	−17			
4640	4651	−11			
4632	4645	−13			

A comparison of these tables shews that, within the limits of common observation, Professor Beetz' remark will apply as well to the agreement of his own numbers as of ours. That the agreement for very dilute solutions is not so good we have already satisfactorily explained.* Professor Beetz did not attempt to measure their conductivity, because he found that when there was less than a certain percentage of salt in his solutions, his method was no longer proof against the effects of polarisation.

I have already said that the only way of testing our method is the comparison of the results which it furnishes with those of a known perfect method. But the comparison to which I refer is not such an one as Professor Beetz has proposed and executed. Its first condition must be the possession of a standard method; its second the elimination of all unnecessary variables. It must be such as to allow suspicion of the cause of differences in results to rest only upon the method itself. If there be x sources of error, it cannot be fastened upon any one. The solutions examined must be the same, the vessels containing them the same, the standards of resistance the same. The only pieces of apparatus which *may*, are those which *must* be changed. The fulfilment of the second condition is easy. The first is generally regarded as fulfilled by the use of Professor Beetz' method. Kohlrausch and

* Page 64 of our paper.

Grotrian used it in testing the validity of their method.* That, however, Professor Beetz' method is free from all the disturbing effects of electrolytic action seems not yet to be thoroughly established. Du Bois-Reymond's experiments show that there is no polarisation. But do Professor Beetz' experiments † prove that new compounds are not formed by the passage of the current, of different conductivity from that of the original liquid? In the tube which contained the electrolyte he placed several pieces of amalgamated zinc, which fitted the tube closely like pistons. Holes were bored through the axis to enable liquid to pass from one side to the other. Generally, he says, the pieces of zinc, so soon as they touched, clung fast to one another (*hafteten fest an einander*), but they could always be separated by inclination of the tube. When all the pieces were lying close together, and one of them close to a zinc electrode, the current would have to pass only once through the electrolyte; but when they were separated from one another, it must several times pass through the solution. If any new resistance were produced there would be several times as much produced in the second as in the first case, and the fact could be observed. This would be conclusive, if it was certain that there was contact between the pieces of zinc lying next one another. If the amalgamated surfaces had been coated with liquid amalgam, there would be sufficient certainty of contact to warrant trust in the experiment. But that does not seem to have been the case, as Professor Beetz does not mention it, and therefore leaves his readers free to suppose that he did not rely upon its agency. He must then have taken for granted that, without the use of any pressure, the pieces of zinc could be brought so close together as to prevent the liquid from penetrating between them, —a somewhat doubtful supposition. If there was a layer of electrolyte, however thin, between each two neighbouring pieces of zinc, electrolysis would occur at as many points of the tube as when the zinc pieces were farther separated, and the resistances observed would be equal whether compounds of greater or less resistance were formed or not. Hence, until more is known of the mode in which Professor Beetz assured himself of the contact of his pistons of zinc, his results must be looked upon as questionable.

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* Pogg. Ann. cliv. p. 9.

† Ibid. cxvii. pp. 6–8.

While in Professor Beetz' comparison his fulfilment of the first condition is not without doubt, his fulfilment of the second is certainly not faultless. He forgets the certainty of error arising from the use of different standards of measurement, as well as the certainty of still greater error from the use of different vessels for holding the electrolyte. He recognises, however, the necessity of knowing that we worked with the same substances, and this fact he proves in a somewhat extraordinary way. His argument takes the form of a hypothetical syllogism:—If we used the same substances, both methods must have indicated the same solution as that of maximum conductivity. Now both methods have done so (this itself was only approximately the case). *Ergo*, we used the same substances. The fallacy is evident. He has mixed up what the logicians call the *modus ponens* with the *modus tollens*, and forgotten that a conclusion can be drawn only from a denied, never from an affirmed "consequent." That Professor Beetz intended the "major premise" as we have given it, is evident, from the fact that, while he could reasonably expect its admission from his readers, he could not expect them to grant the converse, viz., that if both he and we had indicated the same solution as that of maximum conductivity, we must have used the same substances. Such an assumption would make his syllogism correct. But it neglects what the syllogism itself, according to the first version, neglects, viz., the fact of the possible plurality of causes.

While the method which we used cannot, by Professor Beetz' argument, be proved faulty, the results which we published might thus be shown to be unreliable. For that purpose it would be necessary that various methods of acknowledged approximate accuracy should give results approximately the same, and differing widely from ours. Professor Beetz thinks this has been done. "Kohlrausch and Nippoldt have shown how close is the agreement between the results which they, Paalzow, and I have obtained in three quite different ways. The agreement between their measurements of zinc-vitriol solutions and mine, and between their measurements of dilute sulphuric acid and Paalzow's, is perfectly satisfactory" One would understand from these sentences that each of the three experimenters mentioned had used in all his (or their) observations a method quite different from that of the others; and that, while the results of one pair on zinc-vitriol agree, the results of the other pair on dilute sulphuric acid also agree. Kohlrausch and Nippoldt's determinations are used as a medium of connection between those of the other two; and only on the supposition that they used throughout the same method or methods connected by compared results, can they reasonably be used as such, or the agreement between Kohlrausch and Nippoldt and Paalzow be supposed to assist in establishing the accuracy of Professor Beetz' results. What, then, are the facts? Professor Beetz has made a series of observations of the conductivity of zinc-vitriol solutions; Paalzow, of dilute sulphuric acid. Kohlrausch and Nippoldt have investigated solutions of both, *but with different and unconnected methods.* In all cases in which they investigated zinc sulphate they used Professor Beetz' method. The principle of his method is the use of non-polarisable electrodes; that of Kohlrausch and Nippoldt's, the reduction of polarisation by means of rapidly alternating currents and large electrodes to as small an amount as possible. In one determination their mode of investigation was quite the same as Professor Beetz';* in the other they used magneto-electric alternating currents instead of the ordinary galvanic current.† It is evident, however, that, even in this determination, since they used amalgamated zinc electrodes and not platinum ones, they were working with Professor Beetz' method and not their own; for the alternating currents are characteristic of their method, only in so far as they prevent the heaping-up of the polarising substances on platinum or other polarisable electrodes. A link is wanting, then, between the methods used by Kohlrausch and Nippoldt in their sulphuric acid determinations and their zinc sulphate determinations respectively. Nor is this link supplied by the comparison of methods given by Kohlrausch and Grotrian;‡ for they make only a single comparative observation, and their platinum electrode method is an improvement upon that of Kohlrausch and Nippoldt.§ There being no connecting link, Professor Beetz cannot cite

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* Pogg. Ann. cxxxviii. p. 376.

† Ibid. p. 373.

‡ Ibid. cliv. p. 10.

§ Ibid. p. 2.

Paalzow's agreement with Kohlrausch as evidence for his own accuracy, and his "agreement of results obtained by three observers in three quite different ways" becomes *the agreement of results obtained by two observers in the same way.* We must now inquire what even this agreement amounts to. Kohlrausch and Nippoldt made two comparable observations. The first, however,* is rendered worthless by the fact that they assume the resistance of two vessels of the electrolyte, apparently without having made any accurate determination. The second† agrees very well with Professor Beetz' corresponding determination; but in order to conclude from this single agreement to general agreement, the unwarrantable assumption must be made that the error found is not less than the average error.‡ Kohlrausch and Grotrian, whom Professor Beetz also cites, make two comparable observations, but both are questionable from the fact of their being unable to state accurately the constitution of the solutions whose resistance they measured. The authority with which Professor Beetz condemns our results as inaccurate on the ground of non-agreement with the agreeing results of various quite different methods may now be judged by the reader for himself.

That the results of a new method applied by young experimenters should be even approximately accurate is, perhaps, hardly to be expected, and it will probably be found that our numbers are not quite exact. If Professor Beetz' conclusion were well grounded they would need to be corrected only to the extent to which

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* Pogg. Ann. cxxxviii. p. 373.

† Ibid. p. 376.

‡ The "perfectly satisfactory agreement" between Kohlrausch and Nippoldt and Paalzow is based also upon comparison of a single pair of observations, the same unwarrantable supposition being made. With regard to this agreement it is interesting to notice the fact that Kohlrausch has lately corrected his first published numbers to the extent of 4 per cent. Paalzow's observed conductivity instead of being a little more than 2 per cent. less than Kohlrausch's, becomes a little less than 2 per cent. greater. If Paalzow were next to make the same discovery there would still be the same agreement, and Professor Beetz' argument would be untouched. Even if such corrections should proceed alternately, *ad infinitum*, his argument would hold at all stages of the process as well as at the present! It would still be true that Professor Beetz agreed with Kohlrausch and Kohlrausch with Paalzow, and therefore Professor Beetz would be proved to be authoritative. So long as Kohlrausch and Paalzow agree,—it matters not whether in accuracy or in error,—they nevertheless prove Professor Beetz accurate!

Kohlrausch has already corrected his first results to be made "perfectly satisfactory." We might therefore congratulate ourselves on having made so close an approximation, and proceed to lighten our mirror. Whether or not, however, it must be much or little or at all lightened, is a question which must be regarded as not yet settled. It may be that Professor Beetz' results are accurate, and that ours alone need correction, but that is not proved; and, in the meantime, it will be well to hold to the acknowledged truth that neither accuracy nor error is often found to be all on one side.

Professor Beetz "would not have pointed out the weak points of our paper in so searching a style, had we not conducted the experiments under the guidance of Professor Tait," who, in communicating it for us to the Royal Society, took upon himself, he thinks, the responsibility for its contents. He did so, at the very least, to the same extent as is always done by the secretary of a learned Society who communicates a paper not written by a Fellow. The remarks which I have offered will, I venture to think, shew the responsibility to be a lighter matter than Professor Beetz supposes. If our errors were as numerous as his accusations there would certainly be a great deal to answer for. But fortunately the strength of the arguments on which he bases them is inversely as the strength of language in which he expresses them; and as the latter is great, so the former is small.

Monday, 7th June 1875.

DAVID STEVENSON, Esq., Vice-President, in the Chair.

The following communications were read:—

1. On High Flood Marks on the Banks of the River Tweed and some of its tributaries, and on Drift Deposits in Tweed Valley. By David Milne Home, LL.D.

In many parts of Scotland there are indications that our existing rivers reached much higher levels than at present, and that large bodies of water prevailed over districts which are now dry land.

As these facts suggest important speculations as to the physical conditions and climate of the country, it is desirable that they be investigated, before becoming more indistinct from the combined effects of weather and land improvements.

The author divided his paper into the following heads:—

1. Water lines on the banks of the River Tweed and some of its tributaries.
2. Notice of drift deposits, and appearances of ancient lakes.
3. Theoretical explanations suggested.
4. Notice of views by other persons.

I.—*Water Lines on Banks of the Tweed.*

(1.) The lowest, and therefore the most recent, water line is indicated by haugh-lands bounded by a cliff or bank, and at a height above the present ordinary summer level of the river of from 14 to 22 feet, at various places (which were specified) between Berwick and Melrose. The line is low where the floods have room to expand laterally; high, where the banks are near one another and vertical. To the above-mentioned heights the river rose above its present channel on 9th February 1831, being the greatest flood which has occurred during the last hundred years.

(2.) There are two higher water lines, from 22 to 50 feet above the present channel of the river. These being older, they are less continuous and less distinctly marked. With regard to them the question is, were they reached by the river from its present channel, or when its channel was higher?

(3.) Another extensive flat, more or less horizontal, but apparently not produced by the river, is at a height of from 115 to 130 feet above the sea. It is seen on both sides of the Tweed, but not beyond Kelso.

(4.) Traces of two higher flats or terraces exist in the districts adjoining the Tweed Valley—one from 170 to 180 feet, the other from 200 to 220 feet above the sea.

II.—*Notice of Ancient Lakes and Drift Deposits.*

(1.) There are indications that lakes existed formerly in Lauder Valley, at Huntly, near Grant's House, on the north side of Tweed valley; and at Morebattle and Millfield Plain, on the south side of the valley, as well as at many other places.

(2.) The drift deposits consist of clay, gravel, sand, and boulders. The clay generally occupies the lowest parts. Extensive beds of sand exist from the lowest parts up to 1000 feet above the sea and more. Gravel lies more frequently over sand than below it. These deposits form round hills, as also extensive elliptic shaped ridges. These ridges are generally parallel to one another and to the axis of the valley. To the west and north of Kelso their average direction is (by compass) E.N.E; near the sea, about E. and W. There are also, at a level of about 800 feet above the sea, remarkable eskars or kaims, running continuously for more than a mile, and observing approximately a parallelism with hills not far distant. Boulders are of three classes—some from parent rocks situated in the valley, some from rocks in the neighbouring hills, some from rocks in the Highlands.

The localities where striated rocks occur were pointed out.

III.—*Theoretical Explanations.*

The author, to account for the beds of stratified sand and gravel, and for their formation into parallel ridges, as well as for the transportation of boulders, assumes that sea, loaded with ice, prevailed over the district to a height of 1500 feet and more. He infers also that, after the sea began to sink towards its present level, a kyle or arm of open sea prevailed between the Cheviot hills on the south and the Lammermuir range of hills on the north, the shallowest part of which would be the present watershed between the counties of Roxburgh and Dumfries—viz., at St Mary's Loch and Mosspaul, which are about 800 feet above the present sea-level. During the period that the level of this sea continued to sink towards the present level, pauses probably occurred in the process, which would allow of the formation of cliffs by the undermining or erosion of the land, and also the formation of flats or terraces by the deposit of sediment.

A particular account was given of various localities where flat land or terraces in Tweed valley, at various heights above the sea, were recognisable. When the flat of 115 to 120 feet existed, the sea reached to Kelso; and there, at that time, the Tweed would join the sea. As the sea fell—say 30 or 40 feet, to Coldstream—the River Tweed would cut out a deeper channel for itself, and when the sea fell so much more, its channel would be still more deepened, till it reached the present sea-level. During these periods, when the river ran in one channel after another, flood-marks would be made on its banks, traces of which would long remain, though it is only the most recent which can be expected to be now visible.

IV.—*Views of other Persons.*

1. As to the drift deposits—

(1.) Several geologists have ascribed the formation of the parallel ridges of drift deposits in Tweed Valley to the action of land-ice, and suppose that some of these ridges are *lateral*, others *terminal* moraines.

(2.) They have also been ascribed to *fluviatile* action, aiding that theory by the supposition that enormous floods were in former times caused by the climate being more rainy, or by the melting of ice and snow on the hills.

The author combated both views, holding that as similar ridges of sand, gravel, and mud are formed now in the sea, so they may have been formed in this district, when the district was under the sea.

2. As to the high flood-marks on the river, reference was made to the opinions of Mr Alfred Tylor, and of the Rev. Thomas Brown, of this society, as to the probability that those marks were made by the rivers flowing in their existing channels.

The author combated this theory, stating, that when the sea stood at higher levels all the rivers of the country must have likewise flowed in channels at higher levels, and that the flood-marks in question were formed then.

2. Observations on Mr Sang's Remarks relative to the Great Logarithmic Table compiled at the Bureau du Cadastre under the direction of M. Prony. By M. F. Lefort. Communicated by Mr Sang, who has translated the paper from the French.

To the President and Council of the Royal Society of Edinburgh, Scotland.

Paris, *le* 29 *Mars* 1875.

Monsieur le President,

J'ai reçu par une voie détournée, un article de M. Edward Sang, intitulé "Remarks on the great Logarithmic and Trigonometrical Tables computed in the Bureau du Cadastre under the direction of M. Prony." Cet article, qui parait avoir été publié dans les "Proceedings of the Royal Society of Edinburgh, Session, 1874–1875," m'a été adressé à l'Observatoire de Paris. Or je n'ai pas l'honneur d'être astronome. Je suis simplement un inspecteur général des Ponts et Chaussées, ami de la science qu'il a cultivée et qu'il cultive encore dans les trop courts instants de loisir que lui laisse sa carrière professionelle. L'Observatoire de Paris a d'ailleurs bien voulu me renvoyer la brochure à mon domicile, rue du-bac No. 38, à Paris.

Quoique j'aie une opinion faite sur le fond de la controverse qui s'est élevée entre la rédacteur de la feuille périodique, intitulée "Nature" et M. Edward Sang, je crois qu'il ne serait ni opportun, ni convenable que j'exprimâsse un avis. Mais, il est des questions de fait et de doctrine soulevées par M. Sang, qui me touchent trop personellement et intéressent trop la science, pour que je ne regarde pas comme une devoir d'éclairer, dans les limites de mon pouvoir, des savants qui se sont occupés de mes travaux avec tant de bienveillance.

Tel est l'objet, Monsieur le President, de la note ci jointe que je vous prie de vouloir bien soumettre à la Société Royale d'Edinburgh, et de porter à la connaissance de M. Edward Sang, dont le domicile m'est inconnu.

Veuillez agréer, Monsieur le President, l'assurance de ma haute considération et de mes sentiments respectueux.

F. Lefort.

Observations relatives aux remarques publiées par M. Edward Sang dans les "Proceedings of the Royal Society of Edinburgh, Session 1874–1875," sur les grandes tables logarithmiques et trigonomètriques calculées au Bureau du Cadastre sous la direction de Prony; par F. Lefort, Inspecteur général des Ponts et Chaussées, membre correspondant de l'Académie des Sciences de Naples.

M. Edward Sang, dans un article dont je viens de rappeler le titre, a mentionné de la manière la plus flatteuse les travaux que j'ai publiées sur la matière des logarithmes, et notamment sur la grande operation qu'a dirigée Prony à la fin du siècle dernier. Je ne puis que l'en remercier; n'entendant d'ailleurs intervenir en aucun façon dans le fond de la controverse qui s'est engagée entre ce savant et le rédacteur de la feuille scientifique intitulée "Nature." Mais je lui dois, autant qu'à la très honorable Société Royale d'Edimbourg, des explications sur différents points de fait et de doctrine, qu'il regarde comme ressortant de mes écrits, écrits qu'il a mal interprètés, sans doute par suite d'une connaissance incomplète de la langue Française. Je n'ignore pas qu'en rédigeant cette note je m'expose à un danger de même nature; mais je compte sur l'indulgence de M. Sang, comme il peut être assuré de la mienne.

I. M. Edward Sang n'admet pas que les tables de Vlacq, corrigées au moyen de mon *errata*, puissent suppléer les tables nouvelles dont il propose l'impression. Il établit d'abord qu'on ne peut se procurer les tables de Vlacq qu'à un prix élevé; qu'il est encore assez difficile d'avoir un exemplaire du 4^e volume des Annales de l'Observatoire de Paris; enfin, qu'il n'existe pas une concordance parfaite entre les divers exemplaires de l'ouvrage de Vlacq. A l'appui de cette dernière thèse il cite la phrase suivante qui serait imprimée à la page 64 des tables de Taylor; "in about 100 copies; in about 200 copies; doubtful whether a few copies are erroneous or not; in about half the impression; only in one copy; and so on."

Je possède une édition des tables de Taylor, publiée en 1792, à Londres, par les soins de Maskelyne. J'y trouve à la page 64 un errata, avec cette mention fort différente de celle qui précède: "Errata of the logarithmic tables, which affect only part of the impression of the sheet, and have been corrected by the printer since the impression, except any may have escaped correction through inadvertence."

Y-a-t-il eu plusieurs éditions des tables de Taylor? je n'en sais rien. Mais il résulte bien de la préface placée en tête de l'édition que je viens de citer, que la publication a été faite pour la première fois par les soins de Maskelyne. En tout cas, les deux citations me paraissent s'appliquer exclusivement à l'ouvrage de Taylor, et n'avoir aucun trait à l'ouvrage de Vlacq.

L'errata que j'ai donné dans le 4^e volume des annales de l'Observatoire, est relatif à l' "Arithmetica logarithmica . . . . . per Adrianum Vlacq Goudanum, Goudae 1628. Petrus Rammasenius," et non aux contrefaçons qui ont pu se produire.

Quant à l'hypothèse des caractères "moveable types" enlevés par l'ouvrier manœuvrant le tampon à encre "inking dabber," et qui auraient été mal replacées par l'ouvrier qui fait agir la presse "pressman," je ne puis comprendre qu'elle soit produite; car elle suppose l'absence complète des vérifications les plus habituelles, les plus élémentaires, et dont l'omission est d'autant moins probable dans l'espèce que Vlacq, l'auteur de ces tables si précieuses, avait été imprimeur et le prédécesseur immédiat de Petrus Rammasenius.

J'admets bien qu' à la suite des tirages successifs on ait pu rendre plus corrects les exemplaires de Vlacq, mais je n'admets pas qu'on les ait rendus moins corrects. Il est donc possible que l'exemplaire dont on dispose, ne renferme pas toutes les erreurs successivement signalées par Vlacq, Véga, et autres auteurs parmi lesquels je me range. Mais quel danger cela présente-t-il? le nombre des corrections à faire sera moindre, et voila tout.

Je reconnais qu'il n'est pas toujours très facile de se procurer un exemplaire de l'arithmetica logarithmica de Vlacq, et que la rareté de l'ouvrage en rend le prix assez élevé. Tout fois on peut y suppléer avantageusement à l'aide du "Thesaurus logarithmorum completus" de Véga, qui est un ouvrage fort estimable, moins rare et par suite moins cher que le premier. J'ai composé aussi un errata pour les tables de Véga. J'en joins la copie à cette note, et j'en autorise biens volontiers la publication. Mon édition du Thesaurus porte la mention, "Leipzig, 1794."

La difficulté de consulter le 4^e volume des annales de l'Observatoire de Paris, n'est pas sérieuse pour des villes comme Edimbourg, Londres, &c., qui possédent des Bibliothèques publiques et des établissments scientifiques de premier ordre. La copie de mon errata ne demande que quelques heures.

II. J'arrive maintenant au grandes tables du Cadastre et c'est a leur sujet, surtout, que j'éprouve le besoin de rectifier plusieurs des interprétations de M. Sang.

Les observations présentées par M. Le Verrier, dans le séance de l'Académie des Sciences de Paris, en date du 17 Mai 1858, ne résultent pas d'un examen personnel auquel se serait livré le savant directeur de l'Observatoire, mais des conférences que j'ai eu l'honneur d'avoir avec lui. Ainsi que je l'ai dit dans la note présentée à la séance de 21 Mai, "M. Le Verrier avait bien voulu mentionner mes recherches dans la dernière séance." Donc les doutes élevés par ce savant sur la véritable *originalité* des calculs en quelques endroits, n'ont ni plus ni moins de portée que ceux que j'ai exprimés moi-même dans cette séance de 21 Mai, et n'infirment en quoi que ce soit ma conclusion, que je maintiens plus firmement que jamais: "Les tables du Cadastre, comme toutes les œuvres humaines, ne sont donc pas parfaites. Elles ne le sont, ni dans l'exécution, ni, peut-être, dans les détails de la conception. Cependant, elles surpassent de beaucoup, non seulement en étendue mais encore et surtout en correction toutes les tables qui les ont précédées, et les tables plus modernes qui ne lui ont pas été comparées avant la publication."

Le 2^e paragraphe, pag. 12, de la brochure qui m'a été adressée, paragraphe, que je ne reproduis pas à cause de son développement, contient une erreur capitale que je n'ai pas conscience d'avoir fait naître. M. Sang dit qu'il y a un troisième exemplaire des tables "third copy," que avait été laissé à Prony à titre de minute. Je n'ai jamais rien avancé de semblable. Il n'existe en fait que deux exemplaires "two copies" manuscrits des grandes tables du Cadastre. L'introduction de la notice que j'ai publiée dans le tome IV des annales de l'Observatoire, ne laisse aucun doute à cet égard. On y voit comment j'ai été amené, après de longues recherches, a découvrir l'un des *deux* exemplaires que l'on croyait perdu.

En présence de ce fait, il serait inutile que je cherchâsse à combattre les conséquences que tire M. Sang de l'existence d'une transcription: cette transcription est purement imaginaire.

III. M. Edward Sang, mû évidemment par l'unique désir d'obtenir des tables logarithmiques parfaites, met le public savant en défiance contre la valeur réelle des tables du Cadastre. Suivant lui, ces tables n'offriraient de garanties sérieuses que si la méthode suivie était bonne en principe, si elle avait été fidélement exécutée, et si les résultats étaient sincèrement produits.

Sur ces trois chefs "on these three heads," M. Sang se sert de moi comme d'un bélier pour démolir l'édifice construit par Prony, et il croit avoir si bien réussi qu'il lui parait inutile de récapituler les critiques partielles aux quelles il s'est livré. Je pense qu'il aurait conçu une opinion tout autre, s'il lui avait été donné de consacrer quelques années à l'étude d'un travail qui se résume par 19 volumes in folio.

Pour se rendre compte dans tous ses détails de la partie mathématique de la méthode, il faudrait lire l'exposé qu'en a fait Prony dans le volume qui sert d'introduction aux tables. Quoique le mémoire ne soit pas très volumineux, et que j'en aie personnellement pris copie, je n'ai pas trouvé d'imprimeur qui consentit à courir les risques de l'impression. Je me suis alors borné à profiter de la place très honorable, mais restreinte, que M. Le Verrier voulait bien me donner dans l'important receuil qu'il publie, et j'ai cherché, par mon travail personnel, à mettre en saillie tout ce qui m'a paru capital dans l'œuvre de Prony.

Serait-il opportun aujourd'hui de répondre en détail aux critiques de M. Sang? Je l'ignore. En tout cas, le temps me manquerait pour faire une réponse complète. Je dirai seulement que le mystère, "mystery," qui lui parait résulter de l'insuffisance de la collation operée par MM. Letellier et Guyétant sur les tables de Briggs, n'en est pas un pour quiconque a recouru aux sources originales.

IV. Bien que Briggs ait dû à Napier, non seulement l'idée mère des logarithmes, mais même l'idée de leur construction dans le système dont la base est 10, système dont on l'a fort à tort considéré comme l'inventeur et auquel on a donné son nom, j'apprécie à un très haut degré le travail de ce collaborateur de Napier, mais l'étude de l'arithmetica logarithmica m'a permis de reconnaitre des erreurs dans l'établissement même des bases du calcul, et m'a expliqué sur abondamment les fautes qui entachent le grand ouvrage de 1624.

La table de la page 10, "numeri continue medii inter denarium et unitatem" renferme des erreurs, ainsi que cela résulte d'une table analogue, et plus étendue, calculée par Callet.

La table de la page 32, "tabula inventioni logarithmorum inserviens" est également fautive, d'après les travaux de Léonelli et de M. Houël.

On ne peut donc attribuer aux calculs de Briggs, qui reposent sur des bases entachées de quelques erreurs, aucune supériorité sur les calculs effectués au bureau du Cadastre.

Cependant M. Edward Sang va plus loin. Il attaque la méthode des différences mise en usage par Prony, et parait lui préférer les procédés de Briggs, ou ceux que lui-même a récemment employés. Je ne parlerai pas de ces derniers qui me sont inconnus; mais, ayant longuement étudié les procédés de Briggs, et ayant pratiqué moi-même la méthode des différences pour calculer à 7 decimales des tables de logarithmes d'addition et de soustraction, je me crois en droit de combattre les critiques élevées contre cette dernière méthode.

La critique principale de M. Sang est enfermée dans la phrase suivante: "Also an error in the determination of the first difference of the sixth order is augmented 82 472 326 300 times in the final logarithm." En d'autres termes, quand on veut calculer des logarithmes à 14 décimales, en faisant usage de 6 ordres des différences, l'approximation étant portée pour le 1^er^ ordre à 16 décimales, le 2^e^ à 18, le 3^e^ à 20, le 4^e^ à 22, le 5^e^ à 24, et le 6^e^ à 26, l'erreur résultant de l'incertitude sur la valeur de 26^e^ chiffre décimal est multipliée après 200 termes par 82 472 326 300.

Pour voir nettement ce qu'il en est, faisons usage des signes algèbriques. En donnant aux lettres le sens que je leur ai assigné dans mon mémoire inséré au tome IV des annales de l'Observatoire, on a pour la détermination du logarithme final, u_p en fonction du logarithme initial u_0 et des differences successives de u_0 jusqu'au 6^e^ ordre,—

$$u_p = u_0 + p\Delta u_0 + p\frac{p-1}{2}\Delta^2 u_0 + p\frac{p-1}{2}\frac{p-2}{3}\Delta^3 u_0$$

$$+ \ldots \frac{p-3}{4}\Delta^4 u_0 + \ldots . \frac{p=4}{5}\Delta^5 u_0 + \ldots . \frac{p-5}{6}\Delta^6 u_0 .$$

Si l'on désigne par E_0, E_1, E_2, E_3, E , ... , la plus grande erreur que comporte le calcul de u_0, Δu_0, $\Delta^2 u_0$, ... on a, en assignant à toutes les erreurs le même signe, pour la plus grande erreur possible sur le calcul de u_p,

$$\mathrm{E}_p = \mathrm{E}_0 + p\mathrm{E}, + p\,\frac{p-1}{2}\,\mathrm{E}_2 + \,.\,.\,.\,.\,.\; p\,\frac{p-1}{2}\,\frac{p-2}{3}\,\frac{p-3}{4}\,\frac{p-4}{5}\,\frac{p-5}{6}\,\mathrm{E}_5\,.$$

$p = 200$, de sorte que les coefficients successifs ont les valeurs suivantes :

$$200 = 2.10^2,\ 19\,900 \angle 2.10^4,\ 1313\,400 \angle 1,4.10^6$$
$$64\,684\,950 \angle 1.10^8,\ 2535\,650\,040 \angle 3.10^9,$$
$$82\,408\,626\,3000 \angle 1.10^{11}:$$

Ainsi, la plus grande erreur finale sur la valeur de u_{200}, déterminée par la méthode des différences, sera moindre que

$$0{,}0^{14}5 + 0{,}0^{13}1 + 0{,}0^{14}7 + 0{,}0^{14}5 + 0{,}0^{14}2 + 0{,}0^{15}5 = 0{,}0^{13}395\,;$$

en sorte que par le seul fait de la répétition des erreurs commises sur les éléments du calcul, l'erreur totale ne pourrait s'élever à plus de 4 unités du 14^e ordre décimal. On voit combien est fantasmagorique le chiffre 82 472 326 300 (d'ailleurs inexact), qui est donné par M. Sang.

En fait, l'erreur s'élève en réalité plus haut dans les tables du Cadastre, mais cela a lieu par suite des différences omises. Aussi ai-je dit que les logarithmes ont été calculés avec 14 décimales, mais en vue d'avoir seulement 12 décimales exactes, et cette correction est presque absolument assurée.

V. Loin de moi l'idée d'inculper les intentions de M. Edward Sang: je suis convaincu qu'il n'a eu d'autre but que la recherche de la vérité. Le libéralisme scientifique de l'Angleterre est trop connu, et s'est manifesté, il y a quelques anneés, d'une manière trop honorable par la publication des tables de la lune de Hansen, pour qu'on puisse supposer qu'un savant, appartennant à cette nation, cherche de propos délibéré, à discréditer une grande œuvre Française, sur laquelle il est peu on mal renseigné.

Il n'a pas dépendu, et il ne dépend pas encore de moi de porter plus de lumières sur un sujet qui m'a occupé pendant plusieurs années. En 1857, j'ai présenté à l'Académie des Sciences de Paris un mémoire fort étendu sur la théorie des logarithmes, la construction et l'usage des tables logarithmiques. Dans ce travail, j'ai passé en revue tout ce qui a été fait d'important depuis Napier jusqu'à nos jours. Notamment, j'ai fait connaître avec beaucoup de détails l'œuvre de Briggs, et le monument élevé, sons la direction de Prony, par le burean du Cadastre. Ce serait la matière d'un volume in 4° de 200 pages environ. Je n'ai trouvé personne qui consentit à supporter les frais d'impression.

J'extrairai volontiers de mon travail tout ce qui pourra intéresser les savants, et, pour le prouver, je ne crois pouvoir mieux faire que de joindre à cette notice l'errata que j'ai formé pour le "Thesaurus Logarithmorum Completus de Vega." Je n'ai pas souvenance de l'avoir déja publié.

J'ai composé aussi un errata pour l' "Arithmetica Logarithmica de Briggs," qui contient environs 300 (trois cents) articles; mais sa publication devrait être précédé de quelques détails qu'il m'est impossible de donner aujourd'hui. Je ferai remarquer seulement que M. Sang ne parait pas avoir lu, dans mon mémoire inséré au tome IV des annales de l'Observatoire de Paris, la phrase où j'indique dans quelle mesure étroite la collation des tables de Briggs avec les tables du Cadastre a été faite par MM. Letellier et Guyétant: "La collation operée par MM. Letellier et Guyétant ne porte réellement que sur 12 chiffres. Elle aurait pu être étendue à 14 chiffres pour les dix milles premièrs nombres, dont les logarithmes ont été calculés au bureau de Cadastre avec 19 décimales." Tout le mystère consiste donc en ceci. MM. Letellier et Guyétant n'étaient pas des calculateurs de la 2^e section; et il se sont bornés à comparer le travail de Briggs avec celui qui avait été fait par les calculateurs du bureau de Cadastre—qui, comme eux, appartenaient à la 3^e section.

On sait que Legendre a publié, dans son traité des fonctions elliptiques, les logarithmes à 19 figures, tels qu'ils résultent des calculs faits au bureau du Cadastre, pour les nombres premiers compris entre 1 et 10,000.

L'errata qui suit ne reproduit pas l'errata imprimé à la page XXX du Thesaurus Logarithmorum completus. On suppose que les corrections indiqueés par l'auteur ont déja été faites sur l'exemplaire que le calculateur possède.

ERRATA.

Thesaurus Logarithmorum completus, etc., à Georgio Vega.

Emendatis erroribus ab auctore Semetiposo prius signatis, non nulli infra signati adhuc supersunt.

I.

Magnus Canon Logarithmorum vulgarium.

Locus corrig.	Error.	Correct.	Locus corrig.	Error.	Correct.
Log. 558	90	89	Log. 22 312	2	3
863	8	7	22 877	1	2
869	5	4	22 996	2999	3000
10 033	3	2	23 274	299	300
Diff. 10 032		887	23 492	3	2
Diff. 10 033		845	23 820	2	1
Log. 11 003	29	30	24 156	10	09
Diff. 11 002		724	24 626	9	8
Diff. 11 003		687	25 173	9	8
Differentiarum maculæ, brevitatis causâ, haud ultra adscribuntur: attento lectori patent.			25 524	59	60
			25 586	5	6
			25 707	5	6
			26 004	3	4
			26 188	2	3
Log. 11 240	3	2	26 407	5	4
15 620	6	5	26 642	39	40
17 646	8	9	26 717	4	5
17 647	6	7	26 728	46	26
17 648	0	1	27 291	5	4
17 649	0	1	27 560	3	2
20 071	10	09	27 586	8	9
20 280	6	7	27 861	2	3
20 375	5	4	27 921	7	6
20 645	3	2	28 486	699	700
20 822	2	1	28 680	69	70
20 866	1	0	29 112	5	6
21 245	5	4	29 163	8	9
21 749	2	3	29 226	799	800
21 795	5	4	29 446	7	8
21 904	9	8	29 639	8	7
22 016	7	6	29 703	3	2
22 200	4	5	30 499	6000	5999

Locus corrig.	Error.	Correct.	Locus corrig.	Error.	Correct.
Log. 30 502	8	7	Log. 48 845	40	39
30 728	1	2	48 980	9	8
31 001	2	1	49 047	6	5
31 627	5	6	49 409	1	2
31 653	6	8	50 211	9	8
31 735	6	7	50 414	1	0
31 817	79	80	50 601	7	6
31 919	8	7	50 828	3	2
32 111	5	6	50 937	1	0
32 633	9	10	50 996	5	4
32 672	5	4	51 037	3	2
33 071	23	27	51 096	2	1
33 370	6	7	51 175	4	3
34 037	6	7	51 299	3	2
34 162	4	3	51 388	5	4
34 358	4	3	51 389	7	6
34 664	1	0	51 606	1	0
34 702	4	5	51 607	6	5
34 734	7999	8000	51 820	7	6
35 053	8	9	51 915	4	3
35 298	7	8	52 064	2	3
38 051	9	7	52 533	8	7
38 277	1	2	52 565	8	7
38 321	7	6	52 587	8	7
38 783	3	2	52 620	8	7
39 227	4	5	52 792	3	4
39 802	5	4	52 823	7	6
39 839	7	6	52 986	2	1
40 108	2	3	53 647	8	7
40 127	19	20	53 868	5	4
40 966	6	7	54 026	3	2
41 156	5	6	54 145	1	0
41 227	2	3	54 273	4	3
41 385	6	5	54 419	70	69
42 584	1	2	54 708	3	2
44 121	40	39	54 825	4	3
44 822	2	3	55 010	50	49
45 060	3	4	55 115	8	7
45 231	5	6	55 313	9	8
45 238	3	2	55 618	768	678
45 474	5	4	57 089	8	7
45 549	8	7	57 202	7	6
45 571	8	7	57 486	6	5
45 697	7	6	57 751	8	7
45 725	2	1	58 081	2	1
45 755	6	7	58 214	6	5
46 073	9	8	58 223	2	1
47 162	40	39	58 301	1	0
47 476	1	2	58 858	7	6
48 305	5	4	59 007	1	0
48 614	6	7	59 488	4	3
48 626	8	7	P. 173, col. 0 777	Lin. 5ª	Lin. 6ª

Locus corrig.	Error.	Correct.	Locus corrig.	Error.	Correct.
Log. 60 096	2	3	o g. 73 571	2	1
60 401	8	9	73 655	9	8
60 487	2	1	74 527	6	5
60 704	1	2	74 723	8	7
60 794	2	1	74 733	5	4
61 011	4	3	74 932	5	4
61 157	4	3	74 941	40	39
62 038	5	4	75 149	9	8
62 131	7	6	75 386	2	1
62 173	7	6	75 395	6	5
62 257	4	3	75 560	3	2
62 273	4	3	75 562	4	5
62 933	50	49	75 590	4	3
63 183	9	8	75 613	4	3
63 357	50	49	75 841	8	7
63 887	1	0	75 953	4	3
64 086	5	4	77 047	2	1
64 639	1	0	77 437	6	5
64 661	4	8	77 663	7	6
64 993	40	39	77 944	6	5
65 143	1	0	78 079	5	4
65 185	8	9	78 259	2	1
65 311	5	4	79 447	5	4
65 659	1	0	79 467	1	0
65 946	2	3	79 666	20	19
66 187	7	6	80 060	7	8
66 239	4	3	80 062	8	9
66 423	7	6	80 063	2	3
67 399	30	29	80 090	6	7
69 311	7	8	81 212	60	59
69 457	3	2	81 460	8	7
69 477	5	4	82 951	60	59
69 988	2	1	82 991	7	6
70 019	40	39	83 693	6	5
70 040	3	4	83 803	8	9
70 043	1	0	85 651	9	8
70 066	7	8	85 810	19	20
70 599	6	5	86 688	3	4
71 140	9	8	86 708	90	89
71 306	9	8	86 898	0	1
71 569	0	1	87 634	3	2
71 653	3	4	89 182	7	6
71 764	6	5	89 185	6	7
72 103	9	8	90 625	5	6
72 675	5	4	91 086	8	7
73 046	90	89	91 087	7	6
73 059	4	5	93 155	1	2
73 286	2	3	93 498	0	1
73 303	90	89	96 981	80	79
73 404	6	5	97 674	5	6
73 501	9	8	98 336	5	6
73 570	1	0	98 337	49	50

Locus corrig.		Error.	Correct.
Log.	98 338	3	4
	98 339	6	7
	98 340	39	40
	98 341	1	2
	98 342	3	4
	98 345	6	7
	98 346	6	7
	98 348	5	6
	98 350	2	3
	98 352	7	8
	98 353	4	5
	98 356	2	3
	98 357	7	8
	98 358	2	3
	98 359	6	7
	98 360	0	2
	98 362	6	7
	98 365	2	3
	98 366	3	4
	98 367	4	5

Locus corrig.		Error.	Correct.
Log.	98 772	8	7
	98 936	7	8
	98 966	4	3
	99 926	2	1
	100 330	3	2

Wolframii tabula Logarithmorum naturalium.

Log.	1 099 7 853	0021 1 9676	0021 5 9686

Magnus Canon Logarithmorum vulgarium trigonometricus.

Log.tan.	0° 30′ 45″	101	201
Cotg.	0° 30′ 45″	899	799

Aliæ maculæ posterius signabuntur.

Paris, *le* 29 *mars* 1875.
F. Lefort.

Translation.

1. Observations relative to Mr Edward Sang's "Remarks on the Great Logarithmic and Trigonometrical Tables calculated in the Bureau du Cadastre under the direction of Prony," published in the Proceedings of the Royal Society of Edinburgh, Session 1874–1875, by M. F. Lefort, Inspecteur General des Ponts et Chaussées, Corresponding Member of the Academy of Sciences of Naples.

Mr Edward Sang, in the above cited article, makes most flattering mention of the works which I have published on the subject of Logarithms, and particularly on the great operations performed in the end of the last century under the direction of Prony. I cannot but thank him, yet without wishing to intervene in any way in the controversy between him and the editor of the scientific periodical "Nature." But I owe it to him, as well as to the honourable Royal Society of Edinburgh, to give explanations on several points of fact and of doctrine which he regards as deducible from my writings; writings which he has misinterpreted, doubtless from an incomplete knowledge of the French language. In preparing the following note I am aware that I am exposed to a danger of the same kind, and therefore count upon Mr Sang's indulgence, as he may assuredly count upon mine.

I. Mr Edward Sang does not admit that Vlacq's Table corrected by help of my *errata* can supply the place of the new tables which he proposes to print. He argues that Vlacq's Tables can only be had at a great price, and that the 4th volume of the "Annales de l'Observatoire de Paris," is not always easily obtainable; and also that there is not a complete agreement among the different copies of Vlacq's work. In support of this thesis he quotes the following phrase, which should be found on the 64th page of Taylor's Tables:—"In about 100 copies; in about 200 copies; doubtful whether a few copies are erroneous or not; in about half the impression; only in one copy; and so on."

I have a copy of Taylor's Tables, published in 1792 at London, under the care of Maskelyne. Therein I find at page 64 an *errata* with this notice, very different from the preceding:—"Errata of the Logarithmic Tables which affect only part of the impressions of the sheet, and have been corrected by the printer since the impression, except any may have escaped correction through inadvertence."

Have there been several editions of Taylor's Tables? I know nothing of it. But the preface to the edition just mentioned shows that the publication was made for the first time under Maskelyne's care. Any way the two quotations seem to me to apply exclusively to Taylor's work, and to have no reference to that of Vlacq.

The *errata* which I have given in vol. 4 of the "Annales de l'Observatoire," refer to the Arithmetica Logarithmica par Adrian Vlacq, Goudanum, Goudæ 1628, Petrus Rammasenius, and not to any spurious copies.

As to the hypothesis of the types drawn out in the working of the "inking dabber," and misplaced by the pressman; I cannot imagine how it could be, because it supposes the complete neglect of the most elementary and usual verifications; an omission much the less likely since Vlacq, the author of these valuable tables, had been a printer, and was the immediate predecessor of Petrus Rammasenius.

I willingly admit that, in the course of successive impressions, the copies of Vlacq may have been made more correct; it is thus possible that a particular copy may not contain precisely all the errors indicated by Vlacq, Vega, and other authors, among whom I may count myself. But what danger is there from that? there are fewer corrections to be made; that is all.

I admit that it is not always very easy to procure a copy of the "Arithmetica Logarithmica" of Vlacq, and that the scarcity of the book enhances its price. However, the want may be advantageously supplied by the Thesaurus Logarithmorum Completus of Vega, a most estimable work, not so rare and therefore not so costly as the other. I subjoin a copy of a list of errors in Vega which I have made, and of which I willingly authorise the publication; my copy of the Thesaurus has the legend Leipzig, 1794.

There can be no serious difficulty in consulting the 4th volume of the "Annales de l'Observatoire de Paris," in such towns as Edinburgh, London, etc., where there are public libraries and scientific establishments of the first order. The copying of my errata is the matter of a few hours.

II. I come now to the Great Tables of the Cadastre, on which subject chiefly I find it necessary to rectify several of Mr Sang's interpretations.

The observations made by M. Le Verrier at the meeting of the Academy of Sciences of Paris, of date 17th May 1858, were not the results of a personal examination made by the Director of the Observatory, but of the conferences which I had the honour of having had with him. As I have said in the note presented at the Meeting of the 24th May:—"M. Le Verrier has been kind enough, at the previous meeting, to mention my researches." Hence the doubts expressed by this philosopher as to the true originality of the calculations in some places, have neither more nor less weight than those which I myself have expressed at this meeting of the 24th May, and do not weaken in the least the conclusion which I maintain more firmly than ever:—"The Tables of the Cadastre, like all human works, are not then perfect; they are so neither in the execution nor perhaps in the details of the conception; nevertheless, they greatly surpass, not only in extent, but yet and above all in correctness, all the tables which have preceded them, as well as the more modern tables which have not been compared with them before publication."

The second paragraph, p. 12, of the pamphlet sent me, which on account of its length I do not quote, contains a capital error for which I cannot admit that I have given cause. Mr Sang says that there is a third copy of the tables which had been allowed to Prony by way of minutes. I have never said anything of the kind. There only exist, in fact, two manuscript copies of the Great Tables of the Cadastre. The introduction and notice which I have published in vol. 4 of the Annals of the Observatory leave no doubt on that subject. It may be seen therein how, after long researches, I was led to discover that one of the two which was believed to have been lost.

It is, therefore, unnecessary for me to seek to controvert the consequences which Mr Sang has drawn from the existence of an imaginary transcription.

III. Mr Edward Sang, evidently led by the sole desire to obtain perfect logarithmic tables, would have the learned world to mistrust the Cadastre Tables. According to him, these afford no serious guarantee that the principle of the method was good, that these principles were faithfully carried out, or that the results were sincerely given. On these three heads Mr Sang uses me as a battering ram to demolish the edifice erected by Prony, and thinks he has so well succeeded that it was unnecessary to recapitulate the special criticisms which he had made. I think that he would have formed quite a different opinion if he had been privileged to spend years in the study of a work which fills nineteen folio volumes.

In order to give an account of all the mathematical details of the method, it would be necessary to read Prony's explanation in the (manuscript) volume forming the introduction to the tables. Although this memoir be not exceedingly voluminous, and although I have personally made a copy thereof, I have not found a printer willing to run the risk of the impression, and have, therefore, been confined to the honourable but restricted space kindly given by

M. Le Verrier in the annals which he publishes; and I have endeavoured by my own exertions to exhibit all of Prony's work that appeared to me to be most important.

I do not know that it would be opportune at present to reply in detail to Mr Sang's criticisms, and need only say, that the mystery which he thinks to result from the insufficient collation of Briggs' Tables by MM. Letellier et Guyétant is no mystery to those who have had recourse to the original sources.

Although it be true that Briggs owes to Napier not only the fundamental idea of logarithms, but also that of the system computed according to the basis 10, of which system Briggs has without reason been held as the inventor, and to which his name has been attached, I appreciate in the highest degree the work of this fellow labourer with Napier. But the study of the Arithmetica Logarithmica has led me to discover errors in the fundamental basis of the calculation, and has superabundantly explained the faults which mar the great work of 1624.

The table in page 10, "Numeri continue medii inter denarium et unitatem," contains several errors, as is seen from an analogous and more extensive table calculated by Callet.

The table on page 32, "Tabula inventioni logarithmorum inserviens" is equally faulty, according to the works of Leonelli and of M. Houël.

We must not then attribute to the calculations of Briggs, founded on pages containing various errors, any superiority over those executed in the Bureau du Cadastre.

For all that, Mr Edward Sang goes farther; he attacks the method of differences made use of by Prony, and seems to prefer to it the processes followed by Briggs, or those which he himself has recently employed. I say nothing about these last, which are unknown to me; but having for long studied Briggs' processes, and having myself practised the method of differences while computing to 7 decimals tables of logarithms of sums and differences, I believe myself to be in a position to repel the attacks on this latter method.

Mr Sang's principal objection is contained in the following phrase:—"Also an error in the denomination of the first difference of the sixth order is augmented 82 472 326 300 times in the final logarithm." In other words, when we wish to calculate logarithms to 14 decimals, making use of 6 orders of differences, the approximation being carried for the 1st order to 16, for the 2d to 18, the 3d to 20, the 4th to 22, the 5th to 24, and the 6th to 26, the error resulting from an uncertainty in the 26th figure is multiplied after 200 terms by 82 472 326 300.

To see exactly the state of matters, let us make use of algebraic signs. Giving to the letters the meaning which I have assigned to them in my memoir inserted in the 4th volume of the "Annales de l'Observatoire," we have for the determination of the final logarithm $u_{\rm p}$ in terms of the initial logarithm and of the successive differences up to the sixth order,

$$u_p = u_0 + p\Delta u_0 + p\frac{p-1}{2}\Delta^2 u_0 + p\,\frac{p-1}{2}\,\frac{p-2}{3}\,\Delta^3 u_0$$

$$+\ \ldots\frac{p-3}{4}\,\Delta^4 u_0 + \ldots\ldots\frac{p-4}{5}\,\Delta^5 u_0 + \ldots\ldots\frac{p-5}{6}\,\Delta^6 u_0\,.$$

If we denote by E_0, E_1, E_2, ... the greatest error which arises in the calculation of u_0, Δu_0, Δu_0 .... we have, on giving the same sign to all the errors, in order to obtain the greatest possible error in the result.

$$E_p = E_0 + pE_1 + p\,\frac{p-1}{2}\,E_2 + \ldots\ldots p\frac{p-1}{2}\,\frac{p-2}{3}\,\frac{p-3}{4}\,\frac{p-4}{5}\,\frac{p-5}{6}\,E_6\,.$$

putting each error in the differences at ½ of a unit in its own last place, and making p = 200, the successive coefficients, have the following values—200 , 19900 , 13 13400 , 64 684 950 , 2 535 650 040 , 82 408 626 300 .

Thus, the greatest final error in the value of u_{200} determined by the method of differences mnst be less than 0·5 + 1·0 + 0·7 + 0·5 + 0·2 + 0·05 = 3·95 in the fourteenth place; so that by the simple repetition of the errors made in the elements of the calculation, the total error can never rise to more than four units in the 14th decimal place. We see thus how fantasmagoric is the number 82 472 326 300 (inaccurate besides) which is given by Mr Sang.

In point of fact, the error really rises higher than this in the Cadastre tables, but that is because of the differences omitted.

Thus, I have said that the logarithms have been calculated with 14 decimals, but with the view only of having 12 exact; and this degree of accuracy is almost absolutely secured.

V. Far be it from me to entertain the idea of blaming the intentions of Mr Edward Sang. I am convinced that he has had no other desire than to reach the truth. The scientific liberality of England is too well known, and has recently been too well shown, by the publication of Hansen's Lunar Tables, to allow us to suppose that a savant belonging to that nation would deliberately seek to discredit a great French work, concerning which he has been ill-informed.

It has not depended, and it will not depend on me, to throw more light on a subject which has occupied me for several years. In 1857 I presented to the Academy of Sciences of Paris a very extensive memoir on the Theory of Logarithms, on the construction, and on the use of Logarithmic Tables. In this work I have reviewed everything important that has been done from Napier's down to our times. Notably, I have explained with many details the work of Briggs, and the monument erected under the direction of Prony by the Bureau du Cadastre. It would make a quarto volume of some 200 pages; I have found no one willing to bear the expense of the impression.

I would willingly extract from my work anything that would interest the learned; and to prove this I do not think I can do better than annex to this note the *errata* which I have compiled for "Vega's Thesaurus Completus." I have no recollection of having published it before.

I have prepared also an *errata* for Briggs' "Arithmetica Logarithmica," which contains about 300 entries, but its publication would need to be accompanied by some details which I am just now unable to give. I would remark only, that M. Sang does not seem to have read in my memoir inserted in tome IV. des Annales de l'Observatoire de Paris the passage in which I point out the limited extent of the comparison of Briggs' tables with those of the Cadastre made by MM. Letellier et Guyétant. "The comparison made by MM. Letellier et Guyétant extended only to 12 figures. It might have been extended to 14 figures for the first ten thousand numbers, whose logarithms had been computed to 19 places at the Bureau du Cadastre." All the mystery lies here. MM. Letellier et Guyétant were not calculators of the second section, and they confined themselves to the comparison of the work of Briggs with that which was done by the computers in the Bureau du Cadastre, who, like them, belonged to the third section.

We know that Legendre has published in his "Treatise on Elliptic Functions" the logarithms of all prime numbers from 1 to 10,000, as obtained from the calculations made at the Bureau du Cadastre.

The following *errata* does not contain the errors printed at page xxx. of the Thesaurus Logarithmorum Completus; it is taken for granted that the errors pointed out by the author have been already corrected on the computer's copy.

Reply to M. Lefort's Observations. By Edward Sang.

From M. Lefort's opening sentence it appears that he had only recently received the copy of my remarks which had been posted to him on 22d December. Perhaps on this account M. Lefort has been hurried in the perusal of my paper, and so has fallen into several mistakes as to my meaning. These will be apparent to any one who peruses the writings, and I shall pass them over entirely, confining myself to the very few points which are essential to the subject in hand. The only extraneous matter to which I shall allude is this, that while M. Lefort has obviously and justly been desirous of upholding the dignity of the Grandes Tables du Cadastre, he has, in the true spirit of an inquirer after truth, clearly and faithfully exhibited even those points which press most sorely on his own position.

While disclaiming any intention to enter into the controversy opened by "Nature," he at once plunges into it in support of the thesis enunciated by the *non nemo* of that periodical—"Almost all the errors found by Mr Sang by means of this table are among those there given by Lefort, and any one who chooses can, without much expenditure of trouble, render his copy of Vlacq all but free from error—*much more accurate than any new table could* POSSIBLY *be.*" In opposition to this gigantic absurdity, I had pointed out the well recognised danger arising from the use of moveable types: M. Lefort denies and yet admits this danger in one sentence:—"J'admets bien qu'à la suite des tirages successives on ait pu rendre plus corrects les exemplaires de Vlacq, mais je n'admets pas qu'on les ait rendus moins corrects." It is enough for my argument that two copies may differ. In the supplementary table to the Errata of Briggs, M. Lefort supplies a strong corroboration of what I advance. The logarithm of 2087 is therein stated as 9 . . instead of 952. The two figures 52 had been drawn out or been broken while the copy "Sainte Geneviève" was being printed; in my own copy they are correctly given.

In order to sustain this dictum of "Nature," we have to suppose the Tables du Cadastre, which were the basis of Lefort's comparison, to be absolutely correct. Now, while composing the remarks made at the first meeting of the Session, I had only access to M. Lefort's papers in the Comptes Rendus; but through his great kindness I am now in possession of a copy of his most valuable paper inserted in the Annales de l'Observatoire de Paris, and am thereby enabled much more satisfactorily to explain the defects of Prony's mode of procedure because an example of the actual work is therein given.

The design was to compute the successive differences of the logarithms, carrying the decimals two places further at each step; and by the summation of these to obtain 200 terms of the logarithmic progression. An error of unit in the last place of each of these differences will produce an effect on the final term, according to the following scale:—

1*st*,	2·00
2*d*,	1·99 00
3*d*,	1·31 34 00
4*th*,	·64 68 49 50
5*th*,	·25 35 65 00 40
6*th*,	·08 24 08 62 63 00

making a total, if all the errors should happen to be in one way, of

6·2862. Wherefore, if each difference have been computed true to the nearest last figure, the maximum error arising from this mode of calculation is 3·1431. M. Lefort, taking into account the maximum possible error in the first logarithm, makes it 3·95, or say four units in the last place.

All this looks exceedingly well, but has not the slightest reference to the matter in hand. In order to obtain such a miserable degree of precision, we have the labour of computing the first difference of each order, and then the toil of writing 6 times 12 times 200, or 14 400 unnecessary figures; for, to make M. Lefort's formula applicable, each difference of each order must be carried to the 26th place.

Prony did not use the method of differences; he used a method of vitiated differences. To show the nature of the vitiation, I transcribe a few lines of the actual work from M. Lefort's example, which, belonging to an advanced part, has only differences of the fourth order.

Nombres.	Logarithmes.	Δ^1	Δ^2	Δ^3	Δ^4
100 800	00346 05321 0951	43084 5563·17	4274 19·79	848·03	2·52
801	00346 48405 6514	43084 1288·97	4274 11·31	848·00	2·52
802	00346 91489 7803	43083 7014·86	4274 02·83	847·97	2·52
803	00347 34573 4818	43083 2740·83	4273 94·35	847·94	2·52

Here we see that the differences, though computed true to the last figure, are only used to the second preceding figure; thus 2·52 is read 3, and the possible error is augmented one hundred times. But this is not all; the difference of any particular order only comes to affect those of lower orders by the accident of some of the to be rejected figures being more or less than 50; so that the final effect cannot be made the subject of calculation. I find nowhere any attempt to estimate the effect of this systematic vitiation, and shall endeavour to supply the want by taking two extreme imaginary cases. In the first case I shall assume each of the initial differences to be 0·49. Proceeding with these according to the method of Prony, we find

p	u	Δu	$\Delta^2 u$	$\Delta^3 u$	$\Delta^4 u$	$\Delta^5 u$	$\Delta^6 u$
0	0	0·49	0·49	0·49	0·49	0·49	0·49
1	0	0·49	0·49	0·49	0·49	0·49	0·49
	...	...	...	...	...	...	...
200	0	0·49	0·49	0·49	0·49	0·49	0·49

giving for u_{200} the value 0, whereas if computed according to the method of successive differences, the result is $u_{200} = 308$.

If, however, we augment the sixth difference by two units in its last place, leaving the other differences unchanged, we get

p	u	$\Delta^1 u$	$\Delta^2 u$	$\Delta^3 u$	$\Delta^4 u$	$\Delta^5 u$	$\Delta^6 u$
0	0	0·49	0·49	0·49	0·49	0·49	0·51
1	0	0·49	0·49	0·49	0·49	0·50	0·51
2	0	0·49	0·49	0·49	0·50	0·51	0·51
3	0	0·49	0·49	0·50	0·51	0·52	0·51
4	0	0·49	0·50	0·51	0·52	0·53	0·51
5	0	0·50	0·51	0·52	0·53	0·54	0·51
6	1	0·51	0·52	0·53	0·54	0·55	0·51
50	45	0·95	0·96	0·97	0·98	0·99	0·51
100	95	1·45	1·46	1·47	1·48	1·49	0·51
150	190	2·41	2·43	2·45	2·47	1·99	0·51
195	327	3·80	3·84	3·78	3·37	2·44	0·51
196	331	3·84	3·88	3·81	3·39	2·45	0·51
197	335	3·88	3·92	3·84	3·41	2·46	0·51
198	339	3·92	3·96	3·87	3·43	2·47	0·51
199	343	3·96	4·00	3·90	3·45	2·48	0·51
200	347	4·00	4·04	3·93	3·47	2·49	0·51

Thus a change of two units in the last place of the 6th difference has caused a change of 347 in the value of u_{200}; whereas, if it had been computed by the method of differences, the change would only have been ·164, and thus the number which M. Lefort has characterised as "phantasmagorique" has yet to be augmented more than two thousand times, and it is possible that this egregiously absurd mode of proceeding may cause an uncertainty of three units in the twelfth place; also it cannot be predicated that this actually exemplified error is the maximum one.

I had treated as a mystery the fact that MM. Letellier et

Guyétant had not noticed the numerous last place errors in the Arithmetica Logarithmica. In regard to this I now find in M. Lefort's paper inserted in the Annales de l'Observatoire the following statement:—"The comparison made by MM. Letellier et Guyétant was really only to 12 figures. It might have been extended to 14 figures for the first 10,000 numbers whose logarithms had been computed to 19 places in the Bureau du Cadastre," from which it seems that the object of the comparison was not to correct Brigg's tables but to verify, in so far, the Cadastre tables themselves.

The only other point to which I would refer is as to my mistake concerning a *third copy*. The explanation is simple. In common with many others, I had understood that the two copies of the great tables were deposited in separate libraries. Having read only the papers in the Comptes Rendus, which contain no notice whatever of the loss and recovery of one of these copies, nor of the important service rendered by M. Lefort in that recovery, I naturally regarded the *presentation* to the Academy as that of a third copy. The detail of these matters, interesting to all classes of computers, is contained in the Annales de l'Observatoire, a sectional work consulted by only a limited class. From this paper we learn that one of the two copies, so like as to be hardly distinguishable, had been long amissing, its whereabouts unknown, until M. Lefort, by untiring perseverance, traced it to the possession of the Heirs of Prony, to whom it had been allowed by way of minutes, "Cet exemplaire avait été laissé à Prony à titre de minute." That is to say, the Director had taken away one half of the result of this enormous labour, lessening greatly the value of the remaining half by depriving it of the means of verification; and that the so-called presentation was only the restitution of what should never have been taken away.

I crave leave to add one word in regard to the nineteen-place table. On comparing the logarithms of primes from 1163 to 10007 as given by Legendre in his "Exercises de Calcul Integral," Tome III., with my own to twenty-eight places, it is found that, for primes above 1900, hardly a logarithm is true to the nineteenth place; so much so, that to make a list of the errors would be to make a list of all the primes. The only logarithms above 1900 truly given are those of 2417, 2879, 2903, 6379, 8599, and 9137; and, with the exception of the logarithm of 9479 which is unit in excess, all those erring by less than 10 are in defect. A list of the corrections exceeding 9 is subjoined.

Numb.	Corrn.	Numb.	Corrn.	Numb.	Corrn.
1303,	– 10	4201,	+ 28	6659,	– 2494
1579,	+ 10	4409,	+ 55	6781,	– 45
2003,	+ 13	5233,	+ 10	6827,	– 25
2011,	+ 12	5273,	+ 10	6883,	+ 30
2203,	+ 55	5813,	– 245	7001,	+ 53
2207,	+ 30	6011,	+ 14	7109,	– 295
2633,	+ 13	6037,	+ 10	8011,	+ 10
3307,	+ 55	6269,	+ 15	8069,	– 494
3863,	+ 25	6521,	+ 14	8353,	+ 12
3923,	+ 10	6581,	+ 14	8819,	+ 31
4007,	+ 19	6619,	+ 29	9403,	+ 15

The only error higher than the sixteenth place is in the logarithm of 4603, which should be 93974 instead of 93924.

From this it is obvious that the mechanical part of the work had been carefully performed, but that the computers had been unskilled in the management of the final figures, so as to prevent the accumulation of small errors. The fact that almost all the errors lie in one direction points to the influence of some definite but erroneous bye rule.

Finally, on examining the list of corrections given in vol. iv. of the "Annales de l'Observatoire," by help of which, according to "Nature," Vlacq is to be made "much more accurate than any new table could possibly be," I find between the narrow limits from 20000 to 30000 two omissions, at 24580 and 26699, and two mis-corrections, at 26188 and 29163, in all of which M. Lefort has been misled by errors of calculation made at the Bureau du Cadastre, as is clear from the subjoined logarithms set down true to the 15th place—

24580	·39058 18785 50435
26188	·41810 23322 49959
26699	·42649 49953 49034
29163	·46483 21978 49968

We must therefore, it seems, be careful lest in correcting Vlacq by help of Prony's calculations, we do not put him wrong where he is right.

Postscript by M. Lefort.

Les erreurs signalées sur le 10^{me} chiffre décimal pour les logarithmes des nombres 24580, 26188, 26699 et 29163 sont moindres qu'une unité du 12^{me} ordre décimal. Or M. Lefort, dans son article sur les tables du Cadastre a prévenu que "le 12^{me} chiffre décimal peut être accidentellement en erreur de près d'une unité," page 26.

Monday, 21st June 1875.

The HON. LORD NEAVES, Vice-President, in the Chair.

The following Communications were read:—

1. Note on Electric Resistance of Solutions. By William Durham and P. R. Scott Lang, M.A.

This note contains the results of experiments we have made on the electric resistance of solutions by a method brought under the notice of this Society by Messrs Ewing and M'Gregor, and described in their paper printed in the Transactions, vol. 27, page 51. Our results, so far as they have gone, are as follows:—

1. Resistance of solutions of sodium-chloride, and potassium-chloride, varying in strength from ·002 grains to 4 and 5 grains to 25 cubic inches of water. In these weak solutions the polarization was very little and easily got rid of, and the results satisfactory. On plotting these out in the usual manner, we found the curves described to be hyperbolas, as shown in the diagram, where the ordinates represent the strengths of the solutions and the abscissæ the resistances. Becquerel, in his experiments on this subject, found the hyperbolas to be rectangular for the solutions he used, while Ewing and M'Gregor found theirs not to be rectangular. We find some of our curves to be rectangular and others not. Thus we have—

KCl—not rectangular.

We tried with the same arrangement the resistance of distilled water, and found it to be about 37,000 B.A. units per cubic centimetre; but on carefully distilling water twice, we found the resistance had risen as high as 47,000 B.A. units, showing the great difference the least impurity made.

2. The effects of heat on electric resistance. We experimented on water, sodium-chloride, and potassium-chloride—weak solutions of the two latter. We heated them to about 70° centigrade, and measured the resistance as they cooled. We found, as the temperature fell, the *rate of increase* of resistance increased, and the results, on being plotted, all described rectangular hyperbolas, as shown in the diagram. Since making our experiments we find that Professor Beetz of Munich has been making experiments on the same subject, using zinc electrodes and zinc sulphate, thus avoiding polarization almost entirely. His results and ours agree generally.

3. From some phenomena we noticed we were led to try the effect of varying the strength of the current passing through the solution; and as the result of many experiments we find that, as the strength of the current increases, the resistance seems to diminish. We note the results of two experiments on a weak solution of sodium-chloride and a stronger one of copper-sulphate.

Resistance in Current.	Resistance in Solution.
10 B.A. Units	950 B.A. Units.
100 „	1000 „
1000 „	1150 „
10,000 „	1390 „
10 „	132 „
100 „	138 „
1000 „	158 „
10,000 „	187 „

We are not prepared as yet to say to what this effect is due. It may be due in some way to the polarization, but we cannot say for certain till we make further experiments. Our thanks are due to Professor Tait for kindly allowing us the use of laboratory and apparatus.

2. On the Circumscribed, Inscribed, and Escribed Circles of a Spherical Triangle. By C. G. Colson, Esq. Communicated by Professor Tait.

In the following paper I propose to investigate expressions for the vector of the following six points of a spherical triangle:—

(1.) Pole of inscribed circle.
(2.) (3.) (4.) Poles of escribed circles.
(5.) Pole of circumscribed circle.
(6.) The orthocentre or intersection of arcs drawn perpendicularly from angles upon the opposite sides.

These vectors will all be found in terms of the vector of the corners of the triangle drawn from the centre of the sphere.

Throughout the investigation α, β, γ will denote the vectors of A, B, C, the corners of triangle ABC, A′B′C′ will represent the polar triangle of ABC (A′ being pole of BC), &c.; α' β' γ' will denote the vectors of its corners; and following the notation usual in spherical trigonometry, a, b, c, A, B, C will denote sides and angles of the triangle; p_1, p_2, p_3, the perpendicular arcs from A, B, C on BC, &c.; R, r, r_1, r_2, r_3 the radii of the circumscribed, inscribed, and escribed circles.

After finding these vectors we proceed to deduce certain well-known results, among others, to find the radius of the circle (analogous to that discovered by Feuerbach in the case of a plane triangle) which touches the inscribed circle and the three escribed circles.

To find the vector of the pole of inscribed circle. Let ρ be the vector (from centre of sphere) of P, the pole of inscribed circle of the triangle ABC. Then we may express ρ as follows:—

$$\rho = x\alpha + y\beta + z\gamma,$$

where xyz are scalars to be determined. Operating by $S.V\beta\gamma$ on the expression, we have

$$S\rho V\beta\gamma = xS\alpha V\beta\gamma.$$

But

$$V\beta\gamma = \alpha' \sin a \;(\alpha' \text{ being the vector of } A'),$$

therefore

$$S\rho\alpha' = xS\alpha\alpha',$$

or

$$\cos PA' = x \cos AA',$$

or

$$x = \frac{\sin r}{\sin p_1}.$$

Similarly

$$y = \frac{\sin r}{\sin p_2}, \quad z = \frac{\sin r}{\sin p_3}.$$

Hence

$$\rho = \sin r \left(\frac{\alpha}{\sin p_1} + \frac{\beta}{\sin p_2} + \frac{\gamma}{\sin p_3} \right) \quad . \quad . \quad (1.)$$

To find the vectors of the poles of the escribed circles, let $\rho_1\, \rho_2\, \rho_3$ be the vectors of $P_1\, P_2\, P_3$, the poles of the escribed circles opposite to ABC respectively. Then, as before, we may write

$$\rho_1 = x\alpha + y\beta + z\gamma.$$

Determining the scalars xyz as before, we have

$$x = \frac{\cos P_1A'}{\cos AA'}, \quad y = \frac{\cos P_1B'}{\cos BB'}, \quad z = \frac{\cos P_1C'}{\cos CC'}.$$

By geometry of the figure we see that

$$P_1A' = \frac{\pi}{2} + r_1 \quad P_1B' = \frac{\pi}{2} - r_1 \quad P_1C' = \frac{\pi}{2} - r_1.$$

Hence

$$x = -\frac{\sin r_1}{\sin p_1}, \quad y = \frac{\sin r_1}{\sin p_2}, \quad z = \frac{\sin r_1}{\sin p_3}.$$

Therefore

$$\rho_1 = \sin r_1 \left(-\frac{\alpha}{\sin p_1} + \frac{\beta}{\sin p_2} + \frac{\gamma}{\sin p_3} \right) \quad . \quad . \quad (2.)$$

Similarly we find

$$\rho_2 = \sin r_2 \left(\frac{\alpha}{\sin p_1} - \frac{\beta}{\sin p_2} + \frac{\gamma}{\sin p_3} \right) \quad . \quad . \quad (3.)$$

$$\rho_3 = \sin r_3 \left(\frac{\alpha}{\sin p_1} + \frac{\beta}{\sin p_2} - \frac{\gamma}{\sin p_3} \right) \quad . \quad . \quad (4.)$$

COROLL. :

$$\frac{\rho_1}{\sin r_1} + \frac{\rho_2}{\sin r_2} + \frac{\rho_3}{\sin r_3} = \frac{\alpha}{\sin p_1} + \frac{\beta}{\sin p_2} + \frac{\gamma}{\sin p_3} = \frac{\rho}{\sin r}$$

(a result which is useful further on).

To find vector of the pole of the circumscribed circle, let σ be the vector of Q, the pole of the circumscribed circle. Then since any vector may be expressed in terms of any three other conterminal and not complanar vectors, we may write

$$\sigma = x\alpha' + y\beta' + z\gamma' .$$

Operate now by $S . \alpha$. Then noticing that

$$S\alpha\beta' = 0 \quad S\alpha\gamma' = 0$$

we have

$$S\alpha\sigma = xS\alpha\alpha' ,$$

or

$$\cos AQ = x \cos AA' ,$$

i.e.,

$$x = \frac{\cos R}{\sin p_1} .$$

Similarly

$$y = \frac{\cos R}{\sin p_2} , \ z = \frac{\cos R}{\sin p_3} .$$

Hence

$$\sigma = \cos R \left(\frac{\alpha'}{\sin p_1} + \frac{\beta'}{\sin p_2} + \frac{\gamma'}{\sin p_3} \right) \quad . \quad . \quad (5.)$$

Or since

$$V\beta\gamma = \alpha' \sin a , \ \&c.,$$

we may write

$$\sigma = \frac{\cos R}{\sin a \ \sin p_1} (V\beta\gamma + V\gamma\alpha + V\alpha\beta) \quad . \quad . \quad (5.)$$

Or we might proceed thus—

Since

$$QA = QB = QC ,$$

therefore

$$S\sigma\alpha = S\sigma\beta = S\sigma\gamma ,$$

therefore

$$S\sigma(\alpha - \beta) = 0 , \ S\sigma(\beta - \gamma) = 0 ,$$

therefore

$$\sigma \text{ is } \perp^{r} \text{ plane of chordal } \Delta .$$

Hence

$$\sigma = zV(\beta\gamma + \gamma\alpha + \alpha\beta) .$$

Operate by $S\alpha$. Then

$$S\alpha\sigma = zS\alpha V\beta\gamma = z \sin a S\alpha\alpha' ,$$

therefore

$$z = \frac{\cos R}{\sin a \ \sin p_1} ,$$

or

$$\sigma = \frac{\cos R}{\sin a \sin p_1}(V\beta\gamma + V\gamma\alpha + V\alpha\beta) = \cos R\left(\frac{\alpha'}{\sin p_1} + \frac{\beta'}{\sin p_2} + \frac{\gamma'}{\sin p_3}\right).$$

To find the vector of the orthocentre.

Let ω be the vector of X, the orthocentre of the triangle. Then

$$\omega = x\alpha + y\beta + z\gamma.$$

To determine the scalar, operate as before by $S\alpha'$

$$S\alpha'\omega = xS\alpha\alpha'$$

or

$$\cos XA' = x \cos AA'$$

$$(\text{calling arc } XA = q_1 \quad XB = q_2 \quad XC = q_3)$$

$$\sin(p_1 - q_1) = x \sin p_1,$$

or

$$x = \frac{\sin(p_1 - q_1)}{\sin p_1}, \quad y = \frac{\sin(p_2 - q_2)}{\sin p_2}, \quad z = \frac{\sin(p_3 - q_3)}{\sin p_3}.$$

Hence

$$\omega = \frac{\sin(p_1 - q_1)}{\sin p_1}\alpha + \frac{\sin(p_2 - q_2)}{\sin p_2}\beta + \frac{\sin(p_3 - q_3)}{\sin p_3}\gamma \quad (6.)$$

Or we may proceed as follows, and express ω in terms of α' β' γ'. Let

$$\omega = x\alpha' + y\beta' + z\gamma'.$$

Then

$$S\alpha\omega = xS\alpha\alpha',$$

therefore

$$\cos q_1 = x \sin p_1,$$

therefore

$$x = \frac{\cos q_1}{\sin p_1}, \quad y = \frac{\cos q_2}{\sin p_2}, \quad z = \frac{\cos q_3}{\sin p_3}.$$

Hence

$$\omega = \frac{\cos q_1}{\sin p_1}\alpha' + \frac{\cos q_2}{\sin p_2}\beta' + \frac{\cos q_3}{\sin p_3}\gamma' \quad . \quad . \quad (6'.)$$

Having now found very simple and symmetrical expressions for the vector of these six points, we proceed to apply the results to the solution of various well-known problems.

Ex. (1.) To find the arcual distances between the poles of the circumscribed circle and the inscribed circle, also of the escribed circles.

Taking Q, P, P_1, P_2, P_3 to be these points, and σ, ρ, ρ_1, ρ_2, ρ_3 to be their vectors—

by (5)
$$\sigma = \cos R \left(\frac{\alpha'}{\sin p_1} + \frac{\beta'}{\sin p_2} + \frac{\gamma'}{\sin p_3} \right),$$

by (1)
$$\rho = \sin r \left(\frac{\alpha}{\sin p_1} + \frac{\beta}{\sin p_2} + \frac{\gamma}{\sin p_3} \right),$$

therefore
$$S\sigma\rho = \cos R \sin r \left(\frac{S\alpha'\alpha}{\sin^2 p_1} + \frac{S\beta'\beta}{\sin^2 p_2} + \frac{S\gamma'\gamma}{\sin^2 p_3} \right),$$

(noticing that $S\alpha'\beta = 0$, &c.),
therefore
$$\cos QP = \cos R \sin r \left(\frac{\sin p_1}{\sin^2 p_1} + \frac{\sin p_2}{\sin^2 p_2} + \frac{\sin p_3}{\sin^2 p_3} \right),$$

or
$$\cos QP = \cos R \sin r \left(\frac{1}{\sin p_1} + \frac{1}{\sin p_2} + \frac{1}{\sin p_3} \right).$$

Again, by (2)
$$\rho_1 = \sin r_1 \left(-\frac{\alpha}{\sin p_1} + \frac{\beta}{\sin p_2} + \frac{\gamma}{\sin p_3} \right),$$

therefore
$$\cos P_1Q = -S\sigma\rho_1 = \cos R \sin r_1 \left(-\frac{1}{\sin p_1} + \frac{1}{\sin p_2} + \frac{1}{\sin p_3} \right),$$

and
$$\cos P_2Q = S\sigma\rho = \cos R \sin r_2 \left(\frac{1}{\sin p_1} - \frac{1}{\sin p_2} + \frac{1}{\sin p_3} \right),$$

and
$$\cos P_3Q = -S\sigma\rho_3 = \cos R \sin r_3 \left(\frac{1}{\sin p_1} + \frac{1}{\sin p_2} - \frac{1}{\sin p_3} \right).$$

Adopting the usual notation, $\sin p_1 \sin a =$ &c., $= zn$, we have (see Todhunter's Spherical Trigonometry)

$$\cos PQ = \frac{\cos R \sin r}{2n} (\sin a + \sin b + \sin c)$$

$$\cos P_1Q = \frac{\cos R \sin r_1}{2n} (-\sin a + \sin b + \sin c).$$

&c. &c.

Ex. (2.) To find the arcual distances between the orthocentre and the poles of the inscribed, escribed, and circumscribed circles.

Calling the vector of orthocentre (X) ω, we have from (6)

$$\omega = \frac{\sin(p_1 - q_1)}{\sin p_1}\alpha + \frac{\sin(p_2 - q_2)}{\sin p_2}\beta + \frac{\sin(p_3 - q_3)}{\sin p_3}\gamma,$$

therefore

$$S\omega\sigma = \cos R\left(\frac{\sin(p_1 - q_1)}{\sin^2 p_1} S\alpha\alpha' + \frac{\sin(p_2 - q_2)}{\sin^2 p_2} S\beta\beta' + \frac{\sin(p_3 - q_3)}{\sin^2 p_3} S\gamma\gamma'\right),$$

therefore

$$\cos XQ = \cos R\left(\frac{\sin(p_1 - q_1)}{\sin p_1} + \frac{\sin(p_2 - q_2)}{\sin p_2} + \frac{\sin(p_3 - q_3)}{\sin p_3}\right).$$

Again, from second form of (6)

$$\omega = \frac{\cos q_1}{\sin p_1}\alpha' + \frac{\cos q_2}{\sin p_2}\beta' + \frac{\cos q_3}{\sin p_3}\gamma',$$

therefore

$$S\omega\rho = \sin r\left(\frac{\cos q_1}{\sin^2 p_1} S\alpha\alpha' + \frac{\cos q}{\sin^2 p_2} S\beta\beta' + \frac{\cos q_3}{\sin^2 p_3} S\gamma\gamma'\right),$$

or

$$\cos XP = \sin r\left(\frac{\cos q_1}{\sin p_1} + \frac{\cos q_2}{\sin p_2} + \frac{\cos q_3}{\sin p_3}\right).$$

Similarly from (2) (6) we find

$$\cos XP_1 = \sin r_1\left(-\frac{\cos q_1}{\sin p_1} + \frac{\cos q_2}{\sin p_2} + \frac{\cos q_3}{\sin p_3}\right),$$

and similar expressions for cos XP_2, cos XP_3.

Ex. (3.) To find the volumes of pyramids $OP_1P_2P_3$, OP_1P_2P, &c., where O is the centre of the sphere, in terms of the volume of the pyramid OABC.

We have

$$\rho_1 = \sin r_1\left\{\frac{\gamma}{\sin p_3} - \left(\frac{\alpha}{\sin p_1} - \frac{\beta}{\sin p_2}\right)\right\}$$

$$\rho_2 = \sin r_2\left\{\frac{\gamma}{\sin p_3} + \left(\frac{\alpha}{\sin p_1} - \frac{\beta}{\sin p_2}\right)\right\},$$

therefore multiplying these, and taking the vector of each side, we have

$$V\rho_1\rho_2 = 2\sin r_1 \sin r_2 V.\frac{\gamma}{\sin p_3}\left(\frac{\alpha}{\sin p_1} - \frac{\beta}{\sin p_2}\right),$$

or

$$V\rho_1\rho_2 = 2\sin r_1 \sin r_2\left\{\frac{V\gamma\alpha}{\sin p_3 \sin p_1} - \frac{V\gamma\beta}{\sin p_3 \sin p_2}\right\}.$$

Again

$$\rho_3 = \sin r_3 \left(\frac{\alpha}{\sin p_1} + \frac{\beta}{\sin p_2} - \frac{\gamma}{\sin p_3}\right),$$

therefore

$$S\rho_3 V\rho_1 \rho_2 = 2 \sin r_1 \sin r_2 \sin r_3 \frac{-S\alpha V\gamma\beta + S\beta V\gamma\alpha}{\sin p_1 \sin p_2 \sin p_3},$$

or

$$S\rho_3 \rho_1 \rho_2 = 4 \frac{\sin r_1 \sin r_2 \sin r_3}{\sin p_1 \sin p_2 \sin p_3} S\alpha\beta\gamma .$$

Now

$$-S\rho_3 \rho_1 \rho_2 = 6 \text{ vol. of pyramid } OP_1P_2P_3 = 6V$$

$$-S\alpha\beta\gamma = 6 \text{ vol. of pyramid } OABC = 6V$$

therefore

$$V_4 = 4 \frac{\sin r_1 \sin r_2 \sin r_3}{\sin p_1 \sin p_2 \sin p_3} V .$$

Also

$$S\rho V\rho_1 \rho_2 = 2 \sin r \sin r_1 \sin r_2 \frac{-S\alpha V\gamma\beta + S\beta V\gamma\alpha}{\sin p_1 \sin p_2 \sin p_3},$$

or

$$S\rho\rho_1 \rho_2 = \frac{4 \sin r \sin r_1 \sin r_2}{\sin p_1 \sin p_2 \sin p_3} S\alpha\beta\gamma ,$$

or

$$V_1 = \frac{4 \sin r \sin r_1 \sin r_2}{\sin p_1 \sin p_2 \sin p_3} V ,$$

calling pyramid $OPP_1P_2 = V_1$, &c.

Similarly we find the vols. of pyramids OPP_2P_3, &c., and arrive at this result—

$$V_1 \sin r_3 + V_2 \sin r_1 + V_3 \sin r_2 = 3V_4 \sin r .$$

Ex. (4.) To find the radius of Dr Hart's circle, *i.e.*, the circle which touches the inscribed circle and the three escribed circles.

Let η be the vector of the pole of this circle, κ its angular radius. Then since the circle touches all four circles, we must have, if z be its centre

$$\text{arc } zP_1 = \kappa + r_1 , \quad zP_2 = \kappa + r_2 , \quad zP_3 = \kappa + r_3 , \quad zP = \kappa - r .$$

Hence

$$S\eta\rho_1 = -\cos(\kappa + r_1) = \sin\kappa \sin r_1 - \cos\kappa \cos r_1$$

$$S\eta\rho_2 = -\cos(\kappa + r_2) = \sin\kappa \sin r_2 - \cos\kappa \cos r_2$$

$$S\eta\rho_3 = -\cos(\kappa + r_3) = \sin\kappa \sin r_3 - \cos\kappa \cos r_3 ,$$

therefore

$$S\eta\left(\frac{\rho_1}{\sin r_1}+\frac{\rho_2}{\sin r_2}+\frac{\rho_3}{\sin r_3}\right)=3\sin\kappa-\cos\kappa(\cot r_1+\cot r_2+\cot r_3)\,.$$

But

$$\frac{\rho_1}{\sin r_1}+\frac{\rho_2}{\sin r_2}+\frac{\rho_3}{\sin r_3}=\frac{\rho}{\sin r}\,,$$

therefore

$$\frac{S\eta\rho}{\sin r}=3\sin\kappa-\cos\kappa\,(\cot r_1+\cot r_2+\cot r_3)\,.$$

But

$$S\eta\rho=-\cos(\kappa-r)\,,$$

therefore

$$-\frac{\cos(\kappa-r)}{\sin r}=3\sin\kappa-\cos\kappa\,(\cot r_1+\cot r_2+\cot r_3)\,,$$

therefore

$$4\sin\kappa=\cos\kappa\,(\cot r_1+\cot r_2+\cot r_3-\cot r)\,,$$

therefore

$$\tan\kappa=\frac{\cot r_1+\cot r_2+\cot r_3-\cot r}{4}=\frac{\tan R}{2}\,.$$

3. On some Remarkable Changes, Additions, and Omissions of Letters in Certain Cognate European Words. By the Hon. Lord Neaves.

The subject of comparative philology has always interested scholars, but latterly the study has been carried on in a more scientific manner, and I may also say with more success, than at any former period. One great object in prosecuting the study is to detect the various disguises which words radically the same are apt to assume in different languages or dialects. The great scholars of two centuries ago were fully alive to the importance of this inquiry, and although they sometimes indulged in too great a latitude of conjecture, there is scarcely an etymological affinity now generally admitted of which traces and indications are not plainly to be found in the works of those learned men, and more particularly in the writings of Salmasius, the greatest of them all.

But it cannot be denied that a strong impetus to this science has latterly been given, arising partly from a more extended knowledge of the forms of speech since Europeans began to study the cognate languages of the East. Comparative philology has thus assumed a more definite shape within the last fifty years, as for instance in the law of sound-change first pointed out by Rask, and afterwards confirmed and extended by Grimm.

Other phenomena of change have still more recently been made prominent, and to some of these I now wish shortly to direct attention.

An opinion prevails among several eminent philologians that the letter and sound of *l* did not originally occur in the Aryan family to which our chief European languages belong. Its introduction, if it is not original, is certainly not recent, for it would be difficult to maintain that it has not existed for several thousand years, as it plays so conspicuous a part in the Homeric writings. But it appears that the Zend language—that is, the old Persian or Bactrian—had no such letter as *l*, and that European words which have that sound have frequently Zend forms where *r* supplies the place of *l*. It is said also that in the oldest Indian writings the same peculiarity appears, though the *l* has been freely introduced into the later Sanscrit.

Be this as it may, it must be admitted that there is a great affinity between the smooth and the rough liquids, *l* and *r*, and that they are frequently interchangeable. We see much of this in Greek and Latin, and it is not easy to say that either of the two languages shows a preference for one of those letters over the other. Let us take some plain and undoubted examples:—λειριον, G., = lilium, L.; ρακος, G., = λακος, G., a ragged garment; in connection with which it has been specially observed that the Cretan form of Doric frequently confounded ρ and λ. The terminations -ρος, G., and -lus, L., seem cognate, as in τρομερος and tremulus. In Latin itself we have two terminational forms that seem identical—alis and aris—the use of which seems in a great measure determined by euphony, in this way, that where *l* occurs in the radical word, the termination -aris is used for the sake of variety; and when *r* occurs in the radical, -alis is used. Thus from *populus* comes *popularis*; and from *natura*, *naturalis*.

It is remarkable that in the later Romance languages, *l*, when it is found in Latin, sometimes disappears, and is replaced by *r:* as apostolus, apôtre; epistola, epître; capitulum, chapitre, &c. Indeed *l* does not very well stand its ground in modern times. In Italian it often becomes an *i;* in French it becomes an *u;* and in the lower German dialects, such as our own Scottish, it is similarly changed or lost.

Let us now assume as an interim hypothesis that *r* and *l* are interchangeable in Greek and Latin, and see if that assumption will afford us results that tend to confirm its truth.

The names for the *swallow* in those two languages are respectively χελιδων and hirundo. Upon the hypothesis suggested, χελιδων may be changed into χεριδων; and then, by well-known tendencies of the Latin language, the final ν will be dropped, leaving χεριδω, while an *n* may be inserted before the *d* to strengthen the syllable, as in tu*n*do, tudi; fu*n*do, fudi; fi*n*do, fidi; frago, fra*n*go; tago, ta*n*go, &c. We thus get χερινδω and hirundo, the identity of which is manifest.

Χαλαζα and grando, the words for *hail*, may be assimilated nearly in the same way. Χαλαζα becomes χαραζα: this when contracted becomes χραζα, as χαρις becomes gratia. Z is = to ds or di, and with an inserted *n*, χαραζα is equal to grandia, which is close upon *grando.*

Upon this footing we see the identity or near affinity of κρυπτω and καλυπτω; and with these, perhaps, κλεπτω may be connected.

Κυκλος, the Greek for a wheel or ring, may in its more primitive form be set down as κυκρος, which seems cognate to the Indian form chakra, with the same meaning. But κυκρος with a slight metathesis leads easily to the Latin circus, circulus; and it is again possible that by aspirating and modifying the consonants, circus becomes identified with the Teutonic ring = hring, while κυκλος is thought to be cognate to the Teutonic wheel; so many diversities of form may thus be derived from the same elements of a guttural twice repeated, and a liquid *r* or *l* variously arranged.

Ἑλμινθς, by changing the *l* into *r* and prefixing a digamma, becomes vermis, the relation of the aspirate and digamma being the same as in ἑσπερος and vesper. The Greek ἑδρα would be easily changed in Latin into *sedla*, which by assimilation becomes *sella*. *Balbus* and *barbarus* seem in like manner to be connected, the meaning of *barbarus* being one who speaks unintelligibly.

We may here give an example of the same radical word appearing in two different forms in the same language with diversified but kindred meanings. The Greek ἀμέργω has the general meaning of pressing or squeezing, while ἀμέλγω has the special meaning of pressing out milk from the udder. The first of these has not been adopted by the other European languages, but ἀμέλγω is very widely diffused as *mulgeo* in Latin, and *milk* in the Teutonic languages.

A somewhat similar example may be found in the Greek words γραφω and γλαφω. These two words mean different methods of a kindred operation, that of marking intelligible forms by some sharp or cutting instrument, the one designating the process of writing or painting, and the other that of carving or modelling. Another cognate seems to be γλυφω. But of these words, and some others connected with them, I shall have occasion afterwards to speak more fully.

Examples of the interchange of *r* and *l* might be further multiplied, but those already given will sufficiently illustrate the subject, and direct attention to this mode of discovering latent affinities in words.

It has often been surmised that a similar relation subsists between other liquids, as between λυμφα and nympha, and it seems clear that the Attic dialect frequently changed an ν into λ, as in νιτρον, λιτρον, &c. But this hypothesis has not as yet been sufficiently matured, and I refrain from entering on it.

The next point that I shall notice is the peculiarity that undoubtedly exists, of leaving out or adding an initial sibilant in cognate words, so as partially to disguise them. In some languages combinations of letters are found to be admissible, and even frequent, which are not found in other languages, though nearly allied. Neither Greek nor Latin seems to admit of the initial combinations of *sl* or *sn*, and, accordingly, it is probable that an affinity subsists between words in other European languages which show these combinations, and Latin and Greek words that show no sibilant. Thus laxus may be a cognate of slack, limus of slime, &c. Nix, undoubtedly, is identical with snow, and nervus is supposed to be the same as snare, which is a word for a string.

The Greek makes use of the initial combination *sm*, which the Latin rejects; but the Greek is not constant to that combination of letters, and the *s* is often dropped, so that we have *σμικρος* and *μικρος*, *σμαραγδος* and *μαραγδος*. The word *μειδίαω* does not show any initial *σ*, but it may be conjectured that the *σ* was once there, and has been dropped. The prosody and form of Venus's epithet *φιλομμειδὴς* would be well explained by considering it as a corruption of *φίλοσμειδής*. If we adopt this view, we then establish an affinity between the Greek *μειδιαω* and the English *smile*, which is further supported by the well-known tendency of the Greek δ to become an *l*.

In those words in other languages which have an initial *s* and a consonant, the *s* is often dropped in Latin, while the rest of the word is retained. We see examples of this in comparing Greek and Latin words. Thus we have the Greek *σφαλλω* becoming in Latin *fallo; σφογγος* becoming *fungus;* and *σφενδόνη*, *funda*. The Greek *στέγω* and *στέγος* seem to be identical with *tego* and *tecium*, and in other languages the *s* seems also to be lost, as in the Gaelic teach, the English thatch, and the German decken.

In some cases we find an initial *s* in the Teutonic languages, where it is wanting in Latin and Greek. Thus the Latin taurus and the corresponding Greek *ταυρος* seem to be represented by the Teutonic word steer, of which a diminutive is stirk. The Greek *κειρω* appears in English in the form of shear, an *s* being prefixed, and the other consonant thereby softened. This root in the Teutonic languages is very productive, there being many forms of it connected with the process of cutting or dividing, as shear, share, ploughshare, scar, score, sharp, &c. Probably, also, *short* comes from this source, and fully represents the Latin curtus with a sibilant prefixed. The Latin caveo seems to have a cognate in an Anglo-Saxon word scevian, from which come our common words of shy and shun, for the Latin caveo has completely the idea of shunning or being shy of an object. "Hunc tu Romane caveto." The Latin *carus* with careo may possibly be connected with the English *scarce*, for the radical idea in carus is that of scarcity as adding to the value of a thing, just as the physical dearth adds to the moral dearness of an object; *parco* also in Latin may be cognate with the English spare.

The initial *l* in Latin seems often to disturb the formation of words, and to sacrifice some letter that had preceded it. Besides other examples already referred to, we may notice the word *lien*, which appears to have lost the older initials *sp*, which would identify it with *spleen*. But the most remarkable instance of this is found in the word *lis*, which from the old grammarians we know to have been originally *stlis*. But acting on the principle which identifies *l* and *r*, we see that the original form would give us *strits* instead of *stlis*, and thus we should have the word commencing with the same letters as our Teutonic words strife, sturt, streit, G., &c., which undoubtedly are cognate in their meaning with the Latin *lis*, though this word by a strange metamorphosis has lost all trace of that struggle or violent contention which it really represents.

It is a peculiarity of the Greek language that all words beginning with ρ are supposed to have a prefixed aspirate which is analogous to a Latin sibilant. If we transferred a Greek word of this kind to a Teutonic form, we should prefix a sibilant to the *r*, but as *sr* is not a combination favoured by the Teutonic languages, a *t* might be inserted for the sake of euphony. Upon this footing we may plausibly consider the Greek ῥευμα as identical with the Teutonic *stream*. It is remarkable, however, that the Gaelic has no objection to the initial combination of *sr*, and accordingly we find the word *sruth*, pronounced *srhu*, meaning a stream or current, and occupying an intermediate position between the Greek ρεω, ρευσω, and the Teutonic stream. If we could get over the change of vowel, we might in the same way connect the Greek ῥιν, a nose, with the Gaelic sron.

It seems a remarkable circumstance that in Greek and Latin words beginning with a sibilant and another consonant there is a tendency to confound or corrupt the second consonant so as to change one for another in a somewhat arbitrary way. In Latin, in particular, where the consonant succeeding the sibilant is always a tenuis, one tenuis is frequently changed for another in comparing Greek and Latin words. Thus σπουδη and studium seem equivalent, also perhaps σταδιον and spatium, σκεπω and specio, σκυλευω and spolio.

In conclusion, on this modification of original roots by the adjection of an initial sibilant, I shall revert to a set of words already noticed—viz., γραφω, γλαφω, γλυφω. These words are of analogous meaning; and it is interesting to see whether we can find corresponding and cognate words in Latin. I think the Latin words *scalpo* and *sculpo* are in this situation. These words seem to be formed by prefixing a sibilant to the radical elements of the Greek forms which consist of a guttural γ or *c*, a liquid *l* or *r*, and a labial φ or π. Now these elements, with a slight metathesis, may become γραπ or glap, γαρπ or galp; prefixing an *s* we have scalp and sculpo, which words indicate operations of a kindred kind, that of carving or embossing. Some philologists, of whom Salmasius is one, consider that by a similar process γραφω becomes *scribo*.

I now conclude these few observations, not unconscious that some of the conjectures and speculations which have been ventured may be overstrained, or in some respects mistaken. But I feel considerable confidence that the general views I have expressed, most of which are derived from higher sources than my own opinions, are correct in substance, and are calculated to afford considerable aid in the continuous progress which philology is making.

4. De l'interpolation des fonctions irrationnelles en général, et des fonctions logarithmiques en particulier, à l'aide des tables numériques. Par F. Lefort, inspecteur général des Ponts et chaussées, membre correspondant de l'Acadèmie des Sciences de Naples.

Introduction.

Pour faire un usage intelligent des tables des fonctions irrationelles en général, et des tables de logarithmes en particulier, il est indispensable de conaître les formules fondamentales de l'interpolation, et de savoir se rendre compte du degré d'approximation que l'on peut obtenir dans les différénts cas. Je traite dans ce mémoire ces deux points qui sont à peine indiqués dans la plupart des

ouvrages élémentaires, et qui sont complètement omis dans les introductions les plus développées à des tables d'ailleurs très estimables.

De l'interpolation par le moyen des tables numériques de fonctions irrationnelles.

Une table si étendue qu'elle soit, ne peut contenir, dans les limites de l'approximation qu'elle comporte, toutes les valeurs d'une fonction, puisque cette fonction est susceptible de croître par intervalles infiniment petits, et que ses valeurs successives sont calculées pour des acroissements finis, et généralement assez bornés, de la variable. Cependant, on peut se servir de la table des valeurs inscrites pour déterminer approximativement les valeurs intermédiaires de la fonction, et c'est à la solution de ce problême que s'applique la méthode dite d'interpolation.

On démontre que toutes les fonctions qui entrent dans les tables peuvent, entre certaines limites, être développées en séries convergentes suivant les puissances entières et positives de la variable, à laquelle on donne le nom *d'argument* de la table. Si donc, dans le calcul de ces fonctions, on borne l'approximation à l'ordre n, elles pourront être assimilés à des fonctions rationelles et entieres de cet ordre. Ainsi on aura généralement

$$(1) \qquad u = a_0 + a_1 x + a_2 x^2 + \dots . a_n x \ ;$$

u étant une fonction quelconque de x, et a_0, a, . . . des quantités numériques, positives ou négatives.

Les $n + 1$ coefficients de x sont complétement déterminés, quand on connait $n + 1$ valeurs de u, répondant à $n + 1$ valeurs également connues de x. Par suite, la fonction générale de x doit être considerée comme donnée par cela seul que l'on donne $n + 1$ de ses valeurs locales.

D'un autre côté, pour la même fonction u, on a, par la formule générale des différences finies,

$$(2) \quad u_n = u_0 + n\,\Delta u_0 + n\,\frac{n-1}{2}\,\Delta^2 u_0 + \dots n\,\frac{n-1}{2}\,\frac{n-2}{3}\dots\Delta^n u_0,$$

où n est un nombre entier tel qu'en supposant constant l'accroissement Δx de la variable, $n = \frac{x_n - x_0}{\Delta x}$; on peut donc écrire au lieu de l'équation (2)

$$(3)\quad u_n = u_0 + \frac{x_n - x_0}{\Delta x}\frac{\Delta u_0}{1} + \frac{x_n - x_0}{\Delta x}\left(\frac{x_n - x_0}{\Delta x} - 1\right)\frac{\Delta^2 u_0}{1 \cdot 2} + \ldots$$

Cette équation est du n^e^ degré en x, et elle doit devenir identique avec l'équation (1), quand on fait dans cette dernière $x = x_n$; donc les coefficients des mêmes puissances de x_n et de x sont égaux, et les deux équations ne diffèrent qu'en ce que dans l' une on emploie le symbole x et dans l'autre le symbole x_n. On peut dès lors écrire d'une manière générale.

$$(4)\quad u = u_0 + \frac{x - x_0}{\Delta x}\frac{\Delta u_0}{1} + \frac{x - x_0}{\Delta x}\left(\frac{x - x_0}{\Delta x} - 1\right)\frac{\Delta^2 u_0}{1 \cdot 2} + \ldots$$

formule très diffèrente de la formule (3), attendue que $\frac{x - x_0}{\Delta x}$ n'est plus assujetté à être un nombre entier, mais peut passer par toutes les valeurs comprises entre *o* et *n*.

L'équation (4) permet d'interpoler dans la série des valeurs u_0, u, ... u_n, avec le même degré d'approximation* qui a été adopté pour le calcul de ces premières valeurs. On doit remarquer d'ailleurs que, l'interpolation se faisant toujours entre deux termes consécutifs de la série, et l'origine des indices étant arbitraire, on peut toujours faire ensorte que $\frac{x - x_0}{\Delta x}$ soit moindre que l'unité. Dans ces deux conditions, et si les différences sont petites, la formule (4) devient très convergente, et on peut, dans les applications, borner le calcul à un petit nombre des termes de la série.

Par exemple, si l'on prend x_0 pour point de départ, et que l'on considère x comme exprimé en parties de Δx, on doit poser $x_0 = o$ et $\Delta x = 1$, en sorte que l'interpolation s'opère par la formule très simple

$$(5)\qquad u = u_0 + x\,\Delta u_0 + x\,\frac{x - 1}{2}\,\Delta^2 u_0 + \ldots$$

entièrement de même forme que l'équation (2). Toutefois il ne faut pas perdre de vue que x est une quantité plus petite que 1, une véritable fraction de Δx. L'usage, dans le calcul des différences finies, est de dénoter par des indices croissants les valeurs de la fonction qui répondent à des valeurs successivement croissantes de la variable. De cette manière, les différences premières sont toujours positives, lorsque la fonction croit en même temps que la variable, et négatives dans le cas contraire. Il est indispensable d'avoir ces considérations présentes à l'esprit pour ne pas commettre d'erreurs dans l'application des formules, et surtout pour ne pas leur donner une extension qu' elles ne comportent pas.

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* Nous verrons plus loin sous quelles réserves.

Par exemple, on ne pourrait dans la formule (2) changer u_0 en u_n, et reciproquement; mais on devrait écrire

$$(6) \qquad u_0 = u_n - n\Delta\, u_{n-1} + n\,\frac{n-1}{2}\,\Delta^2\, u_{n-2} + \ldots \pm \Delta^n\, u_0\,;$$

ainsi qu'il est facile de le vérifier.

Lorsque $\Delta^n\, u_0 = \Delta^n\, u_n$, on a encore

$$(7)\ u_0 = u_n - n\Delta u_n + n\,\frac{n+1}{2}\,\Delta^2\, u_n \cancel{+} \ldots \pm n\,\frac{n+1}{2}\,\frac{n+2}{3}\ldots\frac{2n-1}{n}\,\Delta^n\, u_n.$$

Cette équation (7), comparée à l'équation (1) conduit à la suivante

$$(8) \qquad u = u_1 - x\Delta u_1 + x\,\frac{x+1}{2}\,\Delta^2\, u_1 - \ldots$$

qui donne lieu aux mêmes remarques que l'équation (5). La formule (8) permet d'interpoler en partant de la valeur supérieure de la fonction. Ce mode d'interpolation qui est souvent avantageux, n'est pas habituellement suivi: on s'appuie en général sur la formule (5).

La problême de l'interpolation est double: il s'agit de déterminer la fonction connaissant la valeur de la variable, ou de déterminer la variable connaissant la valeur de la fonction. Dans le premier cas, il n'y a qu'à mettre en nombres la formule (5), en cherchant dans les tables les valeurs u_0, Δu_0, &c., qui répondent à l'argument immédiatement inférieur à la valeur de la variable. Dans le second cas, on met l'expression de x sous la forme

$$x = \frac{u - u_0}{\Delta u_0 + \dfrac{x-1}{2}\,\Delta^2\, u_0 + \ldots}$$

et on résout l'équation par des approximations successives, en négligeant d'abord les quantités de l'ordre $\Delta^2 u_0$ et des ordres supérieurs. La vraie valeur de l'argument répondant à la fonction u sera $x_0 + x \Delta u_0$.

Si l'on voulait faire usage de la formule (8), on aurait

$$x = \frac{u_1 - u}{\Delta u_1 - \frac{x+1}{2} \Delta^2 u_0 + \dots};$$

et la vraie valeur de l'argument répondant à la fonction u serait $x_1 - x\Delta x_1$. x est toujours compté à partir de l'argument qui sert de base à l'interpolation.

Du degré d'approximation que permettent les tables usuelles.

On entend par tables usuelles celles qui ne nécessitent pas en général l'emploi des différences secondes.

Les tables usuelles les plus étendues ne donnent que les différences du premier ordre, c'est à dire les Δu. Les plus complètes présentent en outre, sous le titre de parties proportionelles, les produits de Δu par 0,1 ; 0,2 . . . jusqu'à 0,9, ou les produits de Δu par 0,01 ; 0,02 ; . . . jusqu'à 0,99. Elles ne fournissent ainsi que les deux premiers termes $u_0 + x\Delta u_0$ de la formule d'interpolation, et c'est à ces deux termes que, pour les cas ordinaires, on borne l'approximation dans la recherche de u ou de x. On écrit donc, suivant le problême à resoudre, soit

$$u = u_0 + x\Delta u_0; \text{ soit } x = \frac{u - u_0}{\Delta u_0}.$$

Il convient d'être fixé sur l'importance de l'erreur commise par suite de l'omission d'une partie des termes de la formule générale, et par suite de l'inscription incomplète des valeurs de la fonction, de ses différences, et des parties proportionelles.

Une table donnée suppose à l'avance un certain ordre d'approximation admis dans le calcul des valeurs de la fonction. Les nombres inscrits pour ces valeurs doivent être exacts à une demi-unité près de l'ordre du dernier chiffre, soit en plus, soit en moins. Ainsi les tables de logarithmes à 7 décimales doivent donner la valeur des logarithmes à une demi-unité près du 7^e ordre décimal.

D'un autre côté, les différences premières inscrites ne sont pas celles qui résulteraient du calcul direct par la formule

$$(9)\ \Delta u = f'(x)\,\Delta x + f''(x)\,\frac{\Delta x^2}{2} + \&c.,$$

en bornant l'approximation à une demi-unité du dernier ordre de la fonction ; ce sont les différences mêmes des valeurs inscrites de la fonction, valeurs qui peuvent être individuellement en erreur de près d'une demi-unité du dernier ordre, de telle sorte que la différence inscrite peut être en erreur de près d'une unité de cet ordre sur la valuer complète que représente la série (9).

Enfin, les parties proportionelles, quand elles résultent des produits par les neufs premiers nombres, sont au plus données avec les dixièmes de l'unité du dernier ordre : par suite, les produits $x\Delta u_0$ peuvent être en erreur sur la vraie valeur de près d'une unité, même en supposant qu'on ne néglige aucune décimale dans la somme des produits partiels qui les composent.

Cherchons à apprécier l'importance de ces diverses causes d'erreur dans la détermination de la fonction par l'argument, ou de l'argument par la fonction.

Soit E, l'erreur propre résultant de l'omission des différences secondes et des différences des ordres supérieurs, on a évidemment

$$E = x\frac{x-1}{2}\Delta^2 u_0 + x\frac{x-1}{2}\,\frac{x-2}{3}\Delta^3 u_0 + \dots$$

Le maximum numérique du coefficient $x\dfrac{x-1}{2}$ a pour valeur 0,125, et a lieu pour $x = 0{,}5$· Le maximum numérique du coefficient $x\dfrac{x-1}{2}\,\dfrac{x-2}{3}$ a pour valeur 0,064 et a lieu pour $x = 0{,}42$. Le maximum numérique des coefficients qui suivent diminue progressivement, et répond à des valeurs progressivement moindre de x. En conséquence, si la valeur numérique des différences successives des divers ordres diminue d'une manière notable, ce qui a lieu dans les tables de logarithmes, par exemple ; la série qui exprime la valeur de E, est très convergente, et il suffit en général de considérer son premier terme pour apprécier l'erreur qui resulte de l'omission des différences secondes et des différences des ordres superieurs.

Dans les tables vraiment usuelles, bien construites, le produit

$x\,\dfrac{x-1}{2}\,\Delta^2 u_0$, en aucun point de la table à interpoler, n'atteint une demi-unité du dernier ordre de la valeur de la fonction; par suite, l'emploi de la formule d'interpolation $u_x = u_0 + x\,\Delta u_0$ n'entraine pas une erreur d'une demi-unité pour cause d'omission des différences secondes. C'est aussi la limite d'exactitude que comportent les valeur inscrites de la fonction.

Voyons maintenant si pour l'usage de l'interpolation, en supposant toujours l'omission des différences secondes, il y a avantage à préférer les *différences tabulaires*, c'est à dire, les différences entre deux valeurs consécutives de la fonction inscrites dans la table, aux *différences vraies* que donne la formule de Taylor.

Je désigne par u, Δu les valeurs complètes de la fonction et de sa différence première, par T, ΔT les valeurs analogues inscrites dans les tables, par ΔV la différence vraie exprimée à une demi-unité près du dernier ordre de la fonction. On a

$u_x = u_0 + x\Delta u_0$. D'ailleurs $u_0 = \mathrm{T}_0 \pm \alpha_0$; $u_1 = \mathrm{T}_1 \pm \alpha_1$; α_0 et α_1 étant des quantités numériques dont la valeur est inférieure à une demi-unité du dernier ordre de la table.

On peut obtenir la valeur approximative de u_x par les formules suivantes:

$$\mathrm{T}_x = \mathrm{T}_0 + x\Delta\mathrm{T}_0;\ \text{ou}\ \mathrm{V}_x = \mathrm{T}_0 + x\Delta\mathrm{V}_0.$$

Comparons entre elles les valeurs $u_x - \mathrm{T}_x$ et $u_x - \mathrm{V}_x$, et nous aurons ainsi l'importance de l'erreur commise dans les deux cas.

Nous remarquons d'abord que $\Delta\mathrm{V}_0$ et $\Delta\mathrm{T}_0$ ne peuvent différer que lorsque les corrections à faire à T_0 et à T_1 pour avoir u_0 et u_1 sont de signes contraires. La comparaison n'est donc à faire que lorsque

$$u_0 = \mathrm{T}_0 \pm \alpha_0;\ \text{et}\ u_1 = \mathrm{T}_1 \mp \alpha_1;$$

les signes supérieurs étant pris ensemble et les signes inférieurs ensemble. On a alors

$\Delta u_0 = \Delta\mathrm{T}_0 \mp (\alpha_1 + \alpha_0)$; et l'on peut avoir $\Delta\mathrm{V}_0 = \Delta\mathrm{T}_0 \mp 1$, d'où

$\Delta u_0 - \Delta\mathrm{V}_0 = \mp(\alpha_1 + \alpha_0) \pm 1$, on déduit de là

$$u_x - \mathrm{T}_x = u_0 - \mathrm{T}_0 + x(\Delta u_0 - \Delta\mathrm{T}_0) = \pm[\alpha_0(1-x) - x\alpha_1] = \omega'$$

$$u_x - \mathrm{V}_x = u_0 - \mathrm{T}_0 + x(\Delta u_0 - \Delta\mathrm{V}_0) = \pm[\alpha_0(1-x) + x(1-\alpha_1)] = \omega''.$$

x, $1-x$, et $1-a_1$ étant des quantités positives, la première valeur de ω'' (avec le signe $+$) est toujours positive, et la seconde (avec le signe $-$) est toujours négative. Les deux valeurs de ω' sont inversement positives ou négatives suivant que $x \lesseqgtr \frac{a_0}{a_0+a_1}$. Leur maximum numérique, relatif à la variable x, répond aux deux limites 0 et 1 des valeurs de cette variable.

$$\text{Pour } x = 0, \quad \omega' = \pm a_0 \quad \text{pour } x = 1, \omega' = \mp a_1,$$

ainsi la valeur numérique de ω' est toujours plus petite que 0, 5 .

Pour $x = 0$ la valeur de ω'' est la même que celle de ω', mais pour $x = 1$, $\omega'' = \pm (1 - a_1)$; la valeur numérique de ω'' pourrait ainsi l'approcher de 1.

On doit donc préférer T_x à V_x: en d'autres termes, il vaut mieux interpoler avec les différences tabulaires qu'avec les différences vraies.

Les raisonnements précédents supposent que l'on a affectué complétement le produit $x(\Delta u_0 - \Delta T_0)$ car c'est à cette condition seulement que l'on peut remplacer $\Delta u_0 - \Delta T_0$ par $\mp (a_1 + a_0)$. Cependant, en faisant usage des tables des parties proportionelles les plus complètes, telles que celles de Bremiker pour les logarithmes à 7 décimales, on ne calcule en général les produits $x\Delta T_0$ ou $x\Delta V_0$ qu'à une demi-unité du dernier ordre près. Voyons ce que deviennent alors les produits que nous avons considérés.

En général, $x\Delta T_0 = e \pm f$, e étant la partie entière du produit et $f<0{,}5$. Comme ΔV_0 peut différer de ΔT_0 de ∓ 1, on aura alors $x\Delta V_0 = x\Delta T_0 \mp x = e \pm f \mp x$.

Le seul cas à examiner est celui où la valeur numérique de $\pm f \mp x$ est plus grande que 0,5, puisque V_x ne peut différer de T_x que dans ce cas. On aurait ainsi $x\Delta V_0 = e \pm f'$, $f'>0{,}5$.

Au moyen des équations ci-dessus la valeur complète de u_x peut prendre les formes suivantes:

$$u_x = u_0 + x\Delta u_0 = T_0 \pm a_0 + x\Delta T_0 \mp x(a_1 + a_0) = T_0 + e + \omega' \pm f.$$

Si l'on prend pour valeur de u_x, $T_x = T_0 + e$, l'erreur réelle $\omega_r = \omega' \pm f$. Elle est numériquement plus petite que 1.

Si l'on prend au contraire pour u_x, $V_x = T_0 + e \mp x$; l'erreur réelle est exprimée par $\omega'_r = \omega' \pm f \mp x$, et elle peut devenir numériquement plus grande que 1.

Donc, en interpolant avec les différences tabulaires, et réduisant les parties proportionelles à leurs parties entières, on est sur de ne pas commettre une erreur qui s'élève à une unité. L'erreur, au contraire, pourrait être supérieure à une unité, si l'on interpolait dans les mêmes conditions avec les différences vraies réduites à leur partie entière. Telle est la raison qui doit faire préférer, au point de vue des approximations, les différences tabulaires aux différences vraies.* Il y en a d'autres, d'ailleurs, quand on envisage la question sous la rapport de la facilité des inscriptions et des vérifications.

Il est à peine utile de dire que la différence tabulaire à adopter est celle qui existe réellement entre les deux nombres consécutifs qu'il s'agir d'interpoler, et non une différence plus ou moins voisine inscrite en marge de la table.

Pour obtenir avec sûreté la partie entière de $x\Delta T_0$ à une demi-unité près, x étant généralement un nombre de deux chiffres au moins, il faut que la table des parties proportionelles donne les dixièmes, si elle fournit seulement le produit de ΔT_0 par les neuf caractères de la numeration décimale.

Toutes les erreurs que nous venons d'apprécier s'appliquent à la détermination de la fonction à l'aide des valeurs données de l'argument. Il importe aussi de se rendre compte de la manière dont les erreurs qui peuvent entacher l'expression de la fonction et de ses différences pèsent sur la détermination de l'argument. On y parvient, sans entrer dans de longs details de calcul, en remarquant que, pour des amplitudes locales et restreintes, les variations des arguments sont à très peu près proportionelles aux variations des fonctions. Si donc la variation Δu de la fonction répond à la variation Δx de l'argument, pour une variation a de la fonction l'argument variera de $\frac{a}{\Delta u}\Delta x$. Cette variation sera d'autant plus faible que la différence de la fonction sera plus grande.

La même considération sert à apprécier l'étendue de l'erreur, lorsque l'argument qui sert à déterminer la fonction n'est lui-même qu'approximativement connu. Si β représente l'erreur possible sur l'argument, la plus grande erreur, dont puisse être entachée pour cette cause la fonction qu'il détermine, est exprimée par $\frac{\beta}{\Delta x} \Delta u$, et l'on voit qu'inversement à ce qui avait lieu dans le cas précédent, l'erreur est d'autant plus considérable que la différence de la fonction a une plus grande valeur.

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* Il est facile de conclure de ce qui precède que l'on doit également préferer les différences tabulaires pour les ordres supérieures, lorsqu'il est nécessaire d'en faire usage.

Lorsque l'on veut apprécier l'influence totale possible de diverses causes d'erreur sur la détermination d'une quantité, il faut donner le même signe aux erreurs possibles calculées, et les ajouter.

Si l'on ajoute entre elles plusieurs quantités qui ne sont exactement connues qu'entre certaines limites, la plus grande erreur possible de la somme sera égale à la somme arithmétique des plus grandes erreurs de chacun des termes, ensorte que l'expression de cette erreur ne change pas quand il s'agit de différence au lieu d'addition.

Dans la multiplication ou dans la division d'une quantité qui n'est pas exactement connue la plus grand erreur croît ou diminue dans la même proportion que la quantité elle même.

Les principes exposés dans cet article permettent de se rendre compte de l'avantage que peut présenter, suivant les cas, l'emploi de l'une ou de l'autre des formules

$$u_x = u_0 + x\Delta u_0 ; \; u_x = u_1 - x\Delta u_1 .$$

x devient ainsi égal à 0,5 au plus, et on atténue l'erreur possible sur le produit $x\Delta T_0$. Si l'on consentait à employer concurrement ces deux formules, dans les conditions que nous avons définies, on pourrait diminuer de près de moitié l'étendue des tables des parties proportionelles. On ne doit pas se dissimuler toutefois que ce double usage exige la coup d'œil d'un calculateur exercé.

(Extrait d'un mémoire sur la théorie des logarithmes, la construction et l'usage des tables logarithmiques, composé en 1857 et resté inédit.

C'est de ce mémoire qu'à été également extrait l'article sur les grandes tables du Cadastre, publié en 1858, dans le tome iv. des Annales de l'Observatoire de Paris.)

Monday, 5th July 1875.

SIR ROBERT CHRISTISON, BART., Hon. Vice-President, in the Chair.

The following Communications were read:—

1. The Theory of the Causes by which Storms Progress in an Easterly Direction over the British Isles, and why the Barometer does not always indicate real vertical pressure. By Robert Tennent, Esq.

Upwards of three years ago the author laid a paper before two members of the Scottish Meteorological Society. The question taken up was, why horizontal movement takes off vertical pressure; and the conclusion arrived at was, that every such horizontal current, owing to its passage over a resisting surface, and by means of rapid upper currents, caused removal of air and lifting, and thereby diminished pressure. It was inferred that the barometer which represented this was consequently an "effect" and not a cause of wind.

The present remarks will be confined mainly to the mechanical effects of motion and friction,—the important questions of temperature, vapour, rotation, external high and low pressure, &c., not being now considered.

Friction.—This forms a very important element. To it is due the retardation of the surface currents, while the upper currents move more rapidly, being comparatively free and unimpeded. Surface retardation is increased by pressure, which amounts to $8\frac{1}{2}$ tons on every square yard, but this gradually diminishes upwards. Tyndal, by experiment, estimated the mobility of the uppers on Mont Blanc as being twice as great as that of the surface. When the atmosphere is in a state of rest, its columns may be represented as being vertical or upright, but when rapid uppers and retarded surface currents prevail, it may then be regarded as moving in inclined columns at an angle to the surface, and in the direction of the moving force.

Supply.—The inclination of the columns will depend not only on surface friction, but also on the supply of air to the moving currents. This may be sufficient, insufficient, or more than sufficient. The supply to the uppers may differ in amount from that to the surface currents. The position and the distance of the source of supply are also important. This may be vertically or horizontally situated. According to Redfield and others, the horizontal extent of an atmospheric disturbance is often two hundred times greater than its vertical height. The arresting effect of such an extensive surface on the supply drawn over it must be great. But if supply is derived from a vertical source, as in the case of a descending current, much less retardation will take place. Hence when the supply to the surface current is from a horizontal source, great inclination of columns will take place, but when from a vertical source, there will be less inclination. An important difference in the mode of inflow of the different winds will thus exist betwixt those vertically and those horizontally supplied. The former will move freely in nearly upright columns, the latter in columns more or less inclined.

Gradients.—A river flows on an incline, by the amount of which its velocity and volume are regulated; but the river itself exerts a reactionary influence on this incline, which it will tend to pull down and lower, if it is not composed of rigid materials, An aerial gradient is not rigid, it is elastic and mobile, and being thus subject to the reactionary influence of the air which it draws to itself, it will not remain stationary, nor will its incline remain unaltered. Its efficiency and the amount of its slope will therefore depend on the amount of facility with which air inflows to it. If the inflow takes place in vertical columns, little or no reactionary influence or lowering will be produced; but if the inflow is in inclined columns, which therefore produce difficult supply, being from a horizontal source, the tendency will be to pull down and lower the gradient, and thereby remove the source of supply to a greater distance. What thus takes place is popularly expressed by the phrase, that the wind blows itself out, which is in fact accomplished by lowering the gradient, and removing the source of supply to such a distance that it is almost entirely arrested by the extent of the resisting surface over which it is now compelled to pass. Thus a gradient represents not only a motive force, but also a reactionary force which is due to it.

Curve of Outward Propagation.—There are thus two different modes of inflow towards the low central barometer; one is an advantageous, the other is a disadvantageous form. It is by this latter mode that the gradient is lowered. It takes place with inclined columns, resulting from rapid uppers and retarded surface currents. Much of the work of inflow is thus thrown upon the uppers. To enable them to maintain their superior velocity, they themselves must be adequately supplied by the uppers in advance. This is accomplished by outward extension ; they advance forwards to procure the requisite supply from the still atmosphere a-head, which now begins to inflow spirally. It is to this advancing line of removal, that the term "Curve of Outward Propagation" is applied. It may be illustrated thus:—If a river flowing down an incline does so uniformly, and at an equal rate of speed, removal will equal restoration; but if in the lower part of its course, a more rapid removal is inaugurated, while restoration or supply above remains as before, the curve representing the poin at which the increased removal begins to travel upwards will represent the forward movement of this curve of outward propagation or extension.

When a rapid fall of the barometer takes place, if the inflow to the depression so formed assumes an advantageous form, it will fill up at once; but if, as usually takes place, it assumes the disadvantageous form or mode of inflow, instead of filling up, it will open out and extend itself outwards all round, like the undulations produced by a stone thrown into still water. The uniformity of this extension will depend on the uniformity of the motive central inflow. With inflowing winds of different degrees of density, temperature, and moisture, it may be safely asserted that such uniformity of inflow will not occur. A disadvantageous mode of inflow will consequently take place in one segment, with a less disadvantageous mode in another. The first is found in the advancing segment, the latter in the rear.

The effect of this want of uniformity in the mode of inflow will be, that the depression, instead of extending itself uniformly all round, *will shallow itself out in one particular direction,* which is that in which the disadvantageous mode of inflow is found, and where the curve of outward propagation exists. This disadvantageous mode of inflow is increased by the circumstance, that as the uppers are rapid, while the surface winds are retarded, the numerous intermediate layers betwixt these, must all move at different rates of speed attended by much friction and consequent retardation.

The direction assumed by the curve, is therefore one which is *nearly opposite to that of the motive inflow, which produces it.* In a somewhat similar manner within the tropics, oceanic currents are in certain cases produced, moving in nearly an opposite direction to that of the North East trades, on which they depend.

Winds representing the different Modes of Inflow.—On the west segment of a barometric depression, Polar winds prevail, which are dry, cold, and dense, and are fed by descending currents, with a vertical source of supply. They may be regarded as surface winds. On the east segment are found equatorial winds, which are warm, moist, and less dense; they are weakened by their ascending tendency,—they have not so much the character of surface winds, but assume more the character of rapid uppers, and instead of a vertical, have a horizontal source of supply.

Progress.—When a barometic depression is formed, a spiral inflow towards the centre takes place; if this were equally uniform in every direction, the great central fall of the barometer would extend itself all round, gradually diminishing as it proceeds outwards towards the circumference, and lowering the surrounding gradients as it proceeds; but if, as is usually the case, the inflow is not uniform, the depression will then extend itself in one particular direction, in the manner above described. This extension, which is due to the mode of the central inflow, takes place mostly in front, and in an easterly direction: it will there create a scarcity of supply, towards which the low central barometer will advance. What thus takes place may be illustrated in this manner:—Suppose that the ascent of a balloon, situated near the surface of the ground, is retarded, though not arrested, by a chain passing over it. This chain, where it reaches the ground on each side, is not fixed to it, but is laid outwards along its surface, one end extends for a short distance, the other for a considerable distance. Under these circumstances, the ascent of the balloon will not be vertical, but in a direction inclined towards that in which the chain extends for the greatest length over the ground, and where, consequently, it is most difficult to lift, and where the drag is greatest.

In a somewhat similar manner the low central barometer, having by means of the peculiar mode of inflow of winds in that segment caused supply there to be scarce, *will itself move in that direction*, to obtain the requisite supply, which it could not procure if it remained stationary: in so doing, it opens out the depression in front, and is enabled to move forward, provided it is sufficiently supplied in the rear. If high pressure or steep gradients existed in front of one of the segments, progress could not there take place, since supply being there abundant, no lowering of the gradient could take place, nor could it shallow itself out in that direction.

Lifting.—From the greater mobility of the atmosphere in the upper regions, it there moves faster, and hence the air is more easily removed than it is near the surface. The atmosphere may thus be conceived to be divided into a number of spherical concentric layers, each possessing a different rate of speed, slipping or sliding over those underneath with an increasing amount of friction, as their position becomes lower. The upper layers possess two sources of supply—one from a horizontal source, the other from the layers underneath, while the surface layers possess only a horizontal source of supply. The facility with which the uppers are thus supplied, tends in the first instance to increase their speed, but when this has taken place to a certain extent, the source of supply will diminish in amount. This is accompanied by a lowering of the gradient, the effect of which is to remove the source of supply to a greater distance, and increase the diminution, until a point is at last reached in which it is almost entirely arrested. When this begins to take place, the uppers will tend to lift and become detached as it were from the surface, thus causing a partial vacuum near the surface.

Lifting may be illustrated by what takes place on the lee side of a house or wall, over which a strong wind blows, a partial vacuum is here formed. The friction which retards the air when flowing over an extensive horizontal surface, may be represented by a series of such obstructions which enable the air to be more easily carried off and removed than it can be restored. This removal causes a local or partial reduction of pressure, while the real vertical pressure of the atmosphere overhead remains unaltered. The relation which exists betwixt pressure and the speed of the winds is altered as their velocity increases, in a somewhat similar way to that which takes place when the lee way of a ship is practically diminished by an increase in its head way.

It must be observed, that lifting can only take place where scarcity of supply exists. The vacuum formed behind a wall over which the wind blows is due to the fact, that removal is there greater than restoration; for if supply was sufficient no such vacuum could exist.

Water flowing from an orifice in the side of a cistern, which is only a little below its surface level, will fall directly downwards; but if the level is raised much above that of the orifice, the water passing through it will be expelled with considerable force; it will "lift" and take a form approaching to that of a horizontally flowing spout. The great mass of water will accumulate in the upper part of the curve of the spout, and will connect itself with the side of the cistern by a thin film of water, which will now take the place of the large body of water which fell vertically downwards when the pressure was less. This accumulation may be taken to illustrate what takes place in the upper part of the atmosphere, while the thin film may represent the diminished pressure at the surface.

Since scarcity of supply exists in the advancing portion of a progressing depression, it is there that lifting is most highly developed. Copiousness of supply is found in the rear, and hence it is there that lifting is least likely to be found.

Lifting takes place where inequality exists in the movement of the various atmospheric layers, hence for this, among other reasons, mountain heights cannot be measured during the prevalence of strong winds, nor is the reduction of the barometer from considerable heights to sea-level at all to be depended upon. Lifting is always preceded by removal of air; but so far as removal alone is concerned, it is accurately represented by the barometer. The *diminished* pressure at the surface due to lifting is also correctly exhibited by the barometer there placed, but in such cases the barometer fails to exhibit the real vertical pressure due to the mass of the column existing overhead. To ascertain this accurately, observations would require to be taken by a series of barometers placed at different heights, and not very far apart.

This tendency of air to accumulate aloft with the abnormal pressure which accompanies it, will be masked by the greater proportional removal of air which takes place at an upper, as compared with a lower station. This is seen in the observations at Geneva and St Bernard. The relations betwixt the pressure which exists at an upper and a lower station will thus be altered in two ways; first, by the tendency of the air to accumulate aloft, which lowers the surface barometer, while it tends to raise the upper barometer; and secondly, *by the greater proportional removal of air* which takes place aloft, depending on the height of the upper station. For this reason, the surface barometer, although it falls with strong winds, will not fall to the same extent as the upper barometer, where so much removal takes place.

Isobarics.—Lifting takes place in front of an advancing depression where supply is scarce; the pressure there indicated is consequently less than it ought to be. In the rear, where supply is more abundant, and where lifting to the same extent does not take place, the barometer there will more nearly indicate real pressure* than it does in front. Hence an isobar in front is not comparable with the same isobar in the rear. An isobar therefore would require to be corrected all round, but in different degrees; when corrected, it would extend further forward, and be more widened out in the advancing segment where progress takes place. Until such a correction is carried out, no uniformity of inflow, either in point of force or of direction can be expected from the present mode of construction of charts. Instead of isobars, this might be represented by a line or curve of Isorhoics, drawn to represent lines of equal inflow. Such an Isorhoic Curve would neither coincide with lines of equal observed pressure, nor with lines of real pressure.

The Weather Charts are, as at present constructed, drawn from observed pressure, and they are also supposed to indicate *real* pressure. But since the Isobars in front are more under the influence of the dynamical element than those in the rear, real pressure is there represented as being lower than it ought to be.

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* In the subsequent use of the term "real pressure," the meaning to be conveyed is this.—The real amount of pressure due to the height of the atmospheric column overhead, but which may not be correctly indicated by the surface barometer.

If the difficulties attendant upon the construction of a chart of Isorhoics could be overcome, it would exhibit a practical standard of reference as to the real amount of inflow of air, which cannot be ascertained by the present system of Isobarics. The introduction of the dynamical element complicates the forms of the Isobaric curves to such an extent as often to render them absolutely uninterpretable: this is done by creating barometric oscillations, and different modes of inflow in the various winds, which would not take place on a frictionless surface.

Barometer, how it represents Pressure.—It is only when the atmosphere is in a state of perfect rest that the surface barometer exhibits the real amount of pressure due to the column of air overhead, and it is only then that the normal diminution of pressure due to the diminished mass takes place in ascending upwards.

But when the atmosphere is in a state of motion and the upper currents move rapidly, the dynamical element then enters more largely into these, than into the slower moving surface currents. The consequence is, that the surface barometer will no longer indicate real pressure.

Owing to the lowering of the gradient in front, this diminution of surface pressure takes place, most in front of a moving depression, and least in the rear. It is due to lifting, hence the barometer, to a certain extent, represents dynamical or fictitious pressure. In the rear it more nearly indicates static or real pressure. No difference of *real* pressure, therefore, seems necessarily to exist here, setting aside, in the meantime, the effects of condensation, to which the reduction of pressure in the advancing segment is usually attributed. Hence it is to the difference betwixt static and dynamic pressure that progress is due.

When a gradient is lowered by friction, the accompanying lowered barometer is an effect, and in so far as it is so, it is incapable of attracting air. The gradient thus lowered is caused to extend itself forwards, and its accompanying barometer will consequently fall at places which it *would not otherwise have reached;* it will there exhibit fictitious pressure, and what takes place may be explained by the use of the term "falling for sympathy with adjoining barometers."

A very considerable portion of the numerous barometric oscillations, which so constantly take place, are an *effect* due to friction from a resisting surface. On a frictionless surface their amount would be greatly reduced; no difference would then exist betwixt the speed of surface and upper currents; and depressions, with their accompanying disturbances, would then no longer possess a self-moving power,—they would cease to move forward, except, perhaps, in imbedding currents.

Conclusions.—When a barometric depression is formed, the winds inflow spirally towards the centre, but they very seldom do so equally or uniformly. In front they do so with difficulty, owing to the peculiar mode of inflow which there takes place; in the rear, they do so with comparative facility. In these circumstances, the low central barometer cannot remain stationary. It will move forward in that direction in which supply is most scarce, and by so doing, it will be enabled to procure the necessary amount of supply, which it could not have received if it remained stationary. It is in this way that progress takes place. A depression thus possesses within itself a self-moving power. When a barometer begins to fall rapidly, the fall may extend itself uniformly all round over the surrounding area. Such uniformity of extension, however, does not usually take place, except when a depression remains stationary. It generally assumes *some particular direction*, which is that indicated by progress. This is due to the difference betwixt dynamical and statical pressure in front and rear, or, perhaps more frequently, to the difference betwixt the amount of dynamical pressure to be found in these positions.

When the atmosphere is in a state of perfect rest, the barometer then indicates real vertical pressure; but when it is in motion, and the surface currents are retarded by friction, while the dynamical element of motion is introduced in a comparatively larger proportion into the uppers, the process of lifting takes place, by which surface pressure, as indicated by a barometer there placed, is reduced, while the real vertical pressure of the column of air aloft remains unchanged. Hence, in these circumstances, a barometer does not exhibit real vertical pressure.

When horizontal movement takes off vertical pressure, this is accomplished in two ways,—first, by actual removal of air, and secondly, by lifting. Strictly speaking, however, no real horizontal movement takes place.

Addendum.

Isobarics.—As above stated, these do not indicate the existence of real pressure. There are three modes in which a barometric chart may be constructed. It may be made to exhibit real pressure, it may exhibit dynamical pressure, or it may assume the form of isorhoic curves, which will represent the correction of barometric pressure.

1. Charts of Real Pressure.—These are the daily weather charts, and the curves are there drawn through figures of equal observed pressure, but they do not exhibit the effects of the introduction of the dynamical element, and hence do not represent real pressure as they are supposed to do. To enable them to do so graphically, the curves in front must be widened out and extended forwards to such a point that the amount of pressure which they there indicate will correspond with an equal amount of observed pressure in the same isobar in the rear. Let the observed pressure in the rear of the isobar in question amount to 30·00, and let it also be supposed to be real, the front of it on the chart will be exhibited as 30·00. Let the dynamical lowering there, however, amount to *say* 0·20. An isobar, therefore, drawn through an observed and real pressure of 30·00 in the rear, will in front require to be extended forwards and drawn through an observed pressure of 30·20, to make it exhibit one which is real and comparable with that in the rear. All its parts will now be graphically comparable. The comparative wideness of the isobars in front, with the corresponding diminution in the steepness of the gradients there, will thus represent a greater scarcity of supply there than is to be found on charts of the usual construction.

A curve thus drawn, although its different portions are thus rendered graphically comparable, will not be one of isobarics, as it is not drawn through figures of equal observed pressure, nor will it indicate the spots at which real pressure actually exists. This may illustrate the result of the present mode of construction in which the existence of real pressure is assumed.

2. Charts of Dynamical Pressure.—If barometric charts, instead of exhibiting real, are supposed to indicate dynamical pressure, this also can be exhibited graphically, and such portions of its curve also be made comparable by using figures of equal observed pressure, corrected, however, to represent those of equal dynamical pressure through which the curve will be drawn. As in the former case, such a chart will not be one of isobarics; it will be one of isodynamics, and will indicate approximately the spot at which real pressure exists.

Such a chart, therefore, will more nearly represent the real state of pressure than one of the ordinary construction, because in few or no instances, over the area usually embraced by it, is the atmosphere in a state of perfect rest, hence real pressure is not often found to exist. In this instance of graphical delineation to exhibit dynamical pressure, an extension of the isobars will take place, but to a greater extent in front than in the rear, though the extension will not be so great as in the former instance.

3. Curve of Isorhoics.—Such a curve as this, which represents uniformity of inflow in the various segments, besides aiding the correction of the barometer, will also increase the reliability of the gradients on which they depend.

Lifting.—It is generally assumed that the force of the wind depends on the steepness of the gradients, and not on the absolute height of the barometer. In the "English Meteorological Magazine," for June 1869, Strachan, however, shows that strong winds generally are also accompanied by a remarkable reduction of pressure. This takes place mostly with equatorial winds, attended also, as shown in a diagram, with the greatest barometric oscillations. The reason of this is, that these winds are fed from a horizontal source of supply, and are drawn over a resisting surface often of great inequality. Under these circumstances "lifting" takes place. With polar winds, which have a vertical source of supply, removed from proximity to a resisting surface, fewer oscillations take place, and the barometer often rises. Jenyns has shown that, unlike the thermometer, the barometer rarely rises above its mean more than one-half of the amount to which it falls below it. When below the mean, equatorial winds prevail, and the greater range of pressure which then takes place I attribute to the cause above stated.

Note.—In the "Philosophical Magazine" for September 1874, Mr Tylor comes to the conclusion that "the barometer cannot give a true indication of weight when there is motion in the atmosphere."

2. On Electric Images. Professor Tait.

3. Laboratory Notes. By Professor Tait.

a. On the Origin of Atmospheric Electricity.

This was a preliminary notice of the results of a series of experiments devised to test the part played by water-vapour in the production of atmospheric electricity. While water is in the form of vapour it must be electrified by contact with the gases of the atmosphere—as they are by contact with one another. Precipitation of vapour in a receiver, whether produced by cold or by exhaustion, was found to be steadily accompanied with a disengagement of electricity. Further experiments are to be made with receivers of very great capacity.

b. Preliminary Experiments on the Thermal Conductivity of some Dielectrics. By Messrs C. M. Smith and C. G. Knott.

These experiments were suggested by observations on the different lengths of time required, under different circumstances, for telegraph cables to assume the temperature of the water in which they were submerged. The method employed was that known as "Ångström's," which has already been described by Prof. Tait (Proc. R. S. E. 1872–73 p. 55–61); the manner of the application of the method being, however, somewhat modified in these experiments, we will give a short description of it.

The substances to be experimented on were obtained in sheets, from which eight or nine circular discs 3 inches in diameter were cut, and piled one on another so as to form a cylinder. Between the first four discs thermo-electric junctions of fine copper and iron wires were inserted. A $\frac{1}{8}$-in. copper plate, with three small hooks on the circumference, was placed on either end, and the whole, having been slightly compressed in a Bramah press, was tied together with strings stretching from hook to hook. The cylinder thus prepared was surrounded with cotton wool, and placed horizontally in a wooden frame, with one end projecting about a quarter of an inch. Over this end a sheet of tinned iron was then drawn so as to screen all except the copper plate from the heat. The hitherto free ends of the thin copper and iron wires were attached to similar pieces of thick copper wire, and to insure equality of temperature were immersed in small vessels of water, placed in a larger vessel also containing water; the other ends of the thicker wires were then carried to the mercury pools of a commutator, so arranged that the junctions could be thrown singly, and in rapid succession, into the circuit of a Thomson's dead beat mirror galvanometer of about 24 ohms resistance. A further resistance of about 30 ohms was also placed in the circuit. The source of heat was a large vessel of boiling water. From one side of this vessel, which was placed on a movable retort stand, a cylinder, with a flat end $3\frac{1}{2}$ inches in diameter, projected for about an inch and a half.

The method of observation was as follows. The water being kept boiling, the vessel was applied for ten minutes with its flat surface *in contact* with the copper plate, then removed to a distance for ten minutes, then again applied for ten minutes, and so on during the whole of the experiment. After this had been continued for about two hours observations were begun. The galvanometer deflections for each of the three junctions were read every minute, the readings being taken from the coldest to the hottest; 15^s were taken to read the three. These readings were continued till two or three complete periods had been observed after the steady periodic state had been arrived at. The deflections thus obtained were plotted in terms of the time; and from the curves so obtained the necessary calculations were made.

Making use of Fourier's theorem, in the form,—

$$y = A_0 + A_1 \cos \frac{2\pi}{T}t + A_2 \cos 2\frac{2\pi}{T}t \ldots\ldots\ldots$$
$$+ B_1 \sin \frac{2\pi}{T}t + B_2 \sin 2\frac{2\pi}{T}t \ldots\ldots\ldots$$

values of A and B were obtained from the expressions

$$4\sqrt{2}A_1 = y_0\sqrt{2} + y_1 - y_3 - y_4\sqrt{2} - y_5 + y_7$$
$$4\sqrt{2}B_1 = y_1 + y_2\sqrt{2} + y_3 - y_5 - y_6\sqrt{2} - y_7\,,$$

where y_0, y_1, &c., are the measured values of the ordinates of the curve, taken at intervals of $\frac{1}{8}$ of a period, the axis of t being a tangent to two of the vertices, or a line parallel to it. From these values of A_1 and B_1, a and β were calculated, so as to fulfil the conditions

$$a = \sqrt{A_1^2 + B_1^2}\,;\ \beta = \tan^{-1}\left(-\frac{B_1}{A_1}\right).$$

These having been calculated for the curves representing the oscillatory state of temperature at two of the junctions, the value of the conductivity (K) was obtained from the equation

$$\frac{K}{c\rho} = \frac{\pi}{T} \cdot \frac{\tau^2}{\log_e \frac{a}{a'} \times (\beta - \beta')}\,,$$

where $c\rho$ is the water equivalent, T the periodic time, and τ the distance between the two junctions. Care must be taken that $\log \frac{a}{a'}$ is the Naperian logarithm, and that $\beta - \beta'$ is measured in radians.* The unit employed in measuring y is of no importance, but τ and T must be measured in the units in which the answer is required. In the following calculations the units employed are millimetres and seconds.

The substances experimented on in these preliminary investigations were, Siemens' gutta-percha, the same as is used by Messrs Siemens Brothers in the manufacture of their cable core, and Hooper's india-rubber, which is the insulating material used by the

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* The unit angle has been named by Prof. J. Thomson a *Radian*. As there is no surface conduction in these experiments, the two quantities referred to ought to be equal. Their more or less close agreement may be taken as a test of the accuracy of each experiment.

"Hooper" Company. The following are the most important results of the experiments :—

Gutta-Percha, No. I.

τ = ·99 mm.; mean free temperature, 40° C.

Junct.	y_0	y_1	y_2	y_3	y_4	y_5	y_6	y_7	A_1	B_1	a	β
I.	0·0	4·8	11·2	18·6	24·4	20·3	12·9	5·6	− 11·13	− 0·86	11·17	175° 34′
II.	1·7	1·8	8·0	13·4	18·9	18·7	11·7	6·7	− 8·46	− 2·72	8·89	162° 10′

$$\log_\epsilon \frac{a}{a'} = 0{\cdot}22835;\ \beta - \beta'^{*} = 13^\circ\ 24' = 0{\cdot}2345 \text{ radians};$$

$$\frac{K}{c\rho} = 0{\cdot}0479 \text{ mm. secs.}$$

Gutta-Percha, No. II.

τ = 2·04 mm.; mean free temperature, 46° C.

Junctions.	y_0	y_1	y_2	y_3	y_4	y_5	y_6	y_7	A_1	B_1	a	β
I.	2·0	6·9	12·7	18·0	17·2	11·9	6·7	1·7	− 7·59	3·48	8·35	24°45′
II.	0·0	1·6	5·9	11·1	13·0	9·8	5·6	1·6	− 6·37	0·31	6·38	2°47′

$$\log_\epsilon \frac{a}{a'} = 0{\cdot}26915;\ \beta - \beta' = 21^\circ\ 58' = 0{\cdot}38397 \text{ radians};$$

$$\frac{K}{c\rho} = 0{\cdot}0494 \text{ mm. secs.}$$

The third junctions in the gutta-percha did not give reliable results, owing to the very slight temperature variations.

India-Rubber, No. I.

τ = 3·320; mean free temperature, 23° C.

Junctions.	y_0	y_1	y_2	y_3	y_4	y_5	y_6	y_7	A_1	B_1	a	β
I.	2·1	5·4	9·2	11·9	9·7	6·1	2·1	0·3	− 4·08	3·69	5·50	42° 10′
II.	0·1	1·6	4·9	7·8	8·4	6·6	3·7	1·1	− 4·13	0·60	4·17	8° 16′

$$\log_\epsilon \frac{a}{a'} = 0{\cdot}27687;\ \beta - \beta' = 33^\circ\ 54' = 0{\cdot}5923 \text{ radians};$$

$$\frac{K}{c\rho} = 0{\cdot}176.$$

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[*] These values of $\beta - \beta'$ include ·0006 radians, being the equivalent for the 7·5″ lost in reading.

Curve III. is in this experiment not so good as curves I. and II., but the mean result from it makes $\frac{K}{c\rho} = \cdot 257$ with a mean free temperature of 20 C.

India-Rubber, No. II.

$\tau = 2\cdot 480$; mean free temperature, $\left\{\begin{matrix}\text{I.}\\\text{II.}\end{matrix}\right\}$ 30° C; $\left\{\begin{matrix}\text{II.}\\\text{III.}\end{matrix}\right\}$ ° C.

Junctions.	y_0	y_1	y_2	y_3	y_4	y_5	y_6	y_7	A_1	B_1	a	β
I.	9·4	13·7	15·6	13·0	8·2	3·2	0·4	4·7	0·69	7·12	7·15	95° 32′
II.	3·8	7·7	11·2	12·0	9·0	5·1	1·4	0·9	– 2·80	4·87	5·62	60° 06′
III.	0·5	2·7	6·7	9·8	9·4	6·5	3·0	0·8	– 4·49	1·84	4·85	22° 17′

$$\left.\begin{matrix}\text{1.}\\\text{II.}\end{matrix}\right\}\log_\epsilon \frac{a}{a'} = 0\cdot 24078;\ \beta - \beta' = 35^\circ\, 26' = 0\cdot 6190 \text{ radians};$$

$$\frac{K}{c\rho} = 0\cdot 1080\,.$$

$$\left.\begin{matrix}\text{II.}\\\text{III.}\end{matrix}\right\}\log_\epsilon \frac{a}{a'} = 0\cdot 14739\,;\ \beta - \beta' = 37^\circ\, 49' = 0\cdot 6607 \text{ radians}.$$

$$\frac{K}{c\rho} = 0\cdot 1653\,.$$

These experiments seem to show, in the case of the india-rubber, a very marked *increase* in the value of $\frac{K}{c\rho}$ with a decrease of temperature; but, unfortunately, the late period of the session at which the specimens were obtained prevented our repeating the experiments, which probably give too high values. We have not yet been able to obtain values for c, but hope to do so at some future time. The values for ρ are roughly—for gutta-percha, $\rho = 0\cdot 97$; for india-rubber, $\rho = 1\cdot 17$ at the temperature of 18·8° C.

In conclusion, our thanks are due to Prof. Tait for the use of his laboratory, and the kind assistance which he gave us in our experiments; to Prof. Jenkin, through whom we obtained the specimens; and to Messrs Siemens and Hooper for the care with which these specimens were prepared.

4. A Chapter on the Tides. By the Rev. James Pearson, M.A., Vicar of Fleetwood. Communicated by Professor Tait.

5. Farther Researches in very perfect Vacua. By Professors Dewar and Tait.

(*Abstract.*)

The paper commences with an account of various methods of producing very perfect exhaustion of a receiver, especially that recently devised by the authors, in which the absorbent power of charcoal is made use of. An attempt is made to calculate the amount of exhaustion thus producible.

Certain experiments described long ago by Bennett, Mark Watt, and others, and very recently much extended and improved by Crookes, are next referred to; with the results obtained by the authors when repeating them in their charcoal vacua.

By operating with discs of rock-salt and other materials under various circumstances of absorption, the observed phenomena are traced to the unequal heating of the movable parts of the apparatus; and their full explanation is given from the kinetic theory of gaseous pressure.

To confirm this explanation various additional experiments are described—some, in particular, with amorphous sulphur. The amount of radiation from a magnesium lamp, as measured by the pyrheliometer, is shown to be quite consistent with the explanation offered.

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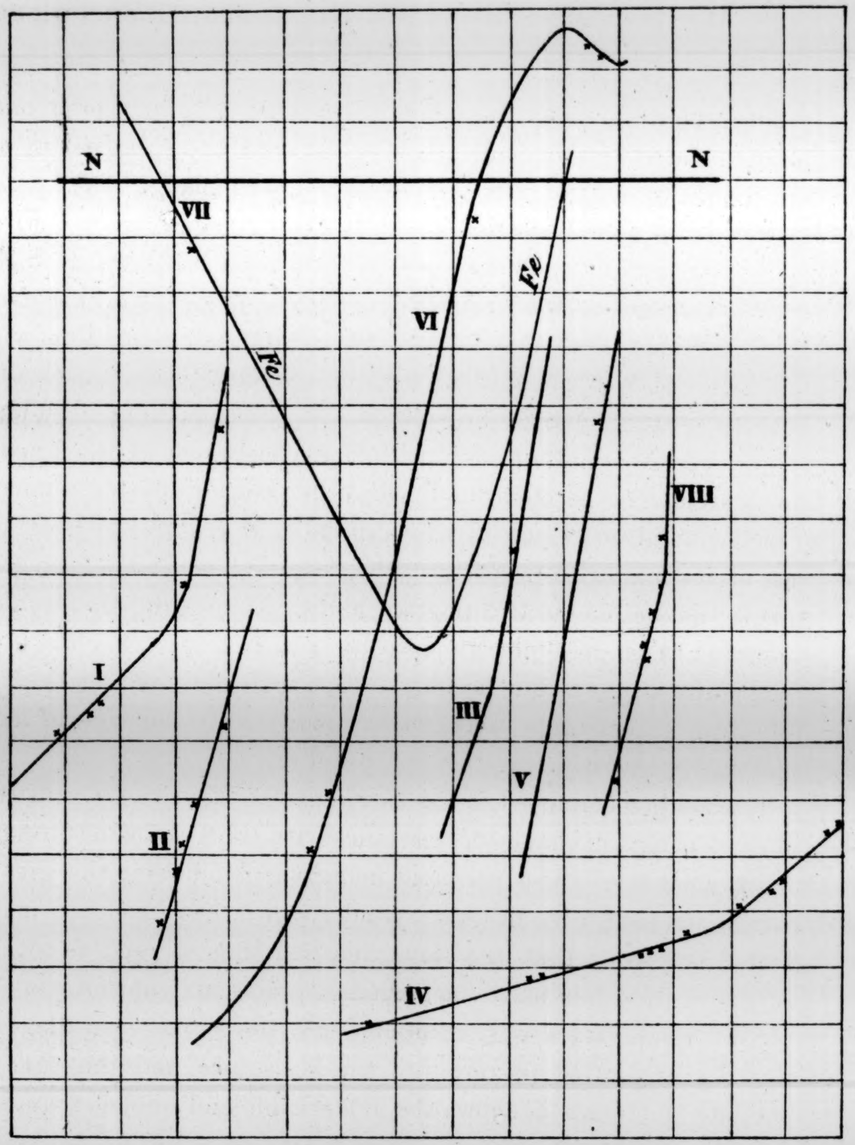

ELECTRIC RESISTANCE OF IRON.

[*Deferred from p.* 491.]

On the Electric Resistance of Iron at a High Temperature.

By Messrs C. M. Smith, C. G. Knott, and A. Macfarlane. (Plate.)

The following paper is a continuation of a former brief one, communicated to the Society, and printed in the *Proceedings*, on the change of electric resistance of iron due to change of temperature. In a note appended to Prof. Tait's paper on a "First Approximation to a Thermo-electric Diagram" (Trans. R. S.E., 1872–73), attention was drawn to the curious phenomenon observed by Gore, that at a temperature about dull red heat, iron wire undergoes sudden changes in length, and also to the further discovery by Prof. Barrett, that if the wire be cooling, a sudden reglow occurs simultaneously with these changes. These phenomena seemed to be connected with other known physical changes which take place in iron at this critical temperature, such as the loss of its magnetic properties, the remarkable bend of the iron line in the thermo-electric diagram, and the interesting alteration in the rate of change of electric resistance with respect to change of temperature, observable in iron at the same dull red heat. The following experiments were made mainly with the view of more thoroughly investigating this last peculiarity.

The method employed in the first series of experiments consisted in comparing the change of resistance with time, the wire throughout the whole of the experiment being surrounded for the greater part of its length by an iron cylinder which had been previously heated to a white heat in a stove, and was then allowed to cool by radiation. By this means a sufficiently slow and uniform alteration of temperature was secured; and the curve (see diagram, Fig. I.) as plotted in terms of the resistance and the time as ordinate and abscissa, shows the remarkable and sudden change of $\frac{dR}{dt}$ at a temperature about the dull red heat—a change observable in all the experiments in connection with the iron wire. Upon the substitution of an equal length of platinum wire for the iron, *ceteris paribus*, it was found that no similar change was observable—the curve obtained (see diagram, Fig. II.) being throughout the same range of temperature very accurately a straight line. The resistance was measured by shunting the current in the galvanometer and battery circuit through the wire under consideration. One great disadvantage of this method is, that the curves do not represent strictly the relation between temperature and resistance, since the rate of cooling is not uniform, and the wire is not at one temperature throughout.

In the second method, however, this difficulty was overcome; for time, as a variable, was eliminated by combining the two originally separate experiments with iron and platinum into one, and comparing the simultaneous changes in the resistances of these wires, which were in exactly similar circumstances. Equal lengths of iron and platinum wire were led side by side through the horizontal cylinder, and their extremities were so connected with the galvanometer and battery circuit, that by simply rocking a six-footed rocker, working in six mercury holes, the current could be shunted through each wire alternately, and thus their resistances could be compared by the effects produced upon the galvanometer.

The curves obtained from these experiments with platinum and iron (see diagram, Fig. III.), their indications being here abscissæ and ordinates, show the same marked change at the same critical temperature. When palladium was substituted for platinum the same peculiarity was observable (see diagram, Fig. IV.); but when palladium was substituted for iron, the curve obtained (see diagram, Fig. V.) was an accurate straight line. It was found expedient, after the first few preliminary experiments, to introduce into the battery circuit a commutator, by which to reverse the current, and so eliminate all errors referable to thermo-electric effects due to the unequal heating (by radiation from the cylinder, or conduction along the heated wires), of the various metallic junctions in the circuit.

In the third distinct series of experiments the arrangement was more elaborate. To the platinum or other wire, whose resistance was compared with that of iron, was attached at the middle point a third wire. By a mechanical arrangement of rockers and commutators, the battery and iron wire could be thrown out of the galvanometer circuit, and thus a thermo-electric standard of temperature was obtained, with which the resistances of the two wires could be at any instant compared. One great drawback in all these experiments was the oxidation of the iron wire. In order to get rid of this to some extent, an entirely new arrangement was devised, in which the heating of the wires was effected by the same current which measured the resistance; but the results obtained by this method were far from satisfactory, owing to the many practical difficulties which were continually cropping up. These experiments were conducted during March and April of 1874.

In the following June, experiments similar to those of the third series above mentioned were made with an iron wire and two platinum-iridium alloys—the same which are called M and N in the thermo-electric diagram. The resistances of M were compared with those of iron, and readings as nearly simultaneous as possible were taken of the deflections due to the M–N thermo-electric junction in the manner described above. Immediately upon the completion of this experiment a triple junction was set up of M, N, and the iron wire already used. The currents due to the Fe–M and M–N junctions were then compared, and, from the curve obtained (see diagram, Fig. VI.), which shows the usual parabolic characters at and near the neutral points, the iron line was laid down with reference to N (Fig. VII.). The features of this line, taken in connection with the M–N deflections observed in the "resistance" experiment, conclusively prove that the bend of the iron line in the thermo-electric diagram occurs at almost exactly the temperature at which the sudden change in the otherwise nearly uniform $\frac{dR}{dt}$ of the same iron wire is observable.

In the diagrams of the various experiments, *all* observed points which do not lie exactly on the curves traced have been inserted.

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ERRATA.

Page 549, line 23, *instead of* "Wheatstone's method (in our reference to which Galvanometer is printed Electrometer)," *read:* "the Electrometer method to which we referred."

Page 549, line 26, *instead of* "Wheatstone," *read:* "this method."

Page 550, line 4 from the bottom, *After* "p. 198" *add* "Also Papers on Electrostatics and Magnetism by Sir William Thomson, 1872, § 350."

Page 555, line 7, *after* "original liquid," *read:* "and that there is no special resistance to the passage of the current from the electrode to the liquid ?"

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